

BRAITHWAITE'S RETROSPECT.

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THE
RETROSPECT OF MEDICINE:

BEING

A HALF-YEARLY JOURNAL

CONTAINING A RETROSPECTIVE VIEW OF EVERY DISCOVERY AND
PRACTICAL IMPROVEMENT IN THE MEDICAL SCIENCES.

EDITED BY

W. BRAITHWAITE, M.D.

LATE LECTURER ON MIDWIFERY AND THE DISEASES OF WOMEN AND CHILDREN
AT THE LEEDS SCHOOL OF MEDICINE, ETC.

AND

JAMES BRAITHWAITE, M.D. LOND.

LECTURER ON DISEASES OF WOMEN AND CHILDREN AT THE
LEEDS SCHOOL OF MEDICINE.

ASSISTANT-SURGEON TO THE LEEDS HOSPITAL FOR WOMEN AND CHILDREN.

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CONTENTS OF VOL. LXXIII.

SYNOPSIS.

PRACTICAL MEDICINE.

DISEASES AFFECTING THE SYSTEM GENERALLY.

ARTICLE.	AUTHOR.	PAGE.
1 Some Considerations respecting Inflammation ...	<i>Dr. C. Handfield Jones</i>	1
2 Inoculability of Specific Fevers	<i>Editor of the British Medical Journal</i>	8
3 Dr. Klein's Report on the Contagium of Enteric Fever	<i>Dr. Klein</i>	9
4 On Typhoid Fever and the Fatal Effects of Digitalis	<i>Dr. Wm. Murrell</i>	12
5 The Use of Saccharated Lime in Typhus Fever and other Complaints	<i>Prof. Cleland</i>	16
6 On Fever	<i>Dr. J. Burdon Sanderson</i>	17
7 Cold Water in Fever	<i>Dr. C. Binz</i>	22
8 Pernicious Fever: Febris Algida and Febris Comatosa	<i>Dr. John Sullivan</i>	23
9 On Salicylic Acid as an Antipyretic	<i>Dr. C. A. Ewald</i>	31
10 On the Treatment of Rheumatic Fever with Salicylic Acid	<i>Editor of the Medical Times and Gazette</i>	33
11 On the Treatment of Acute Rheumatism by Salicin	<i>Dr. T. MacLagan</i>	34
12 On Salicylic Acid	<i>Dr. J. C. Ogilvie Will</i>	40
13 On Diphtheria	<i>Sir John Rose Cormack</i>	44
14 On Diphtheria	<i>Dr. Robert Bell</i>	47
15 The Universal Disinfecting Powder	50
16 On Chloride of Lead as a Deodoriser and Disinfectant	<i>Dr. R. H. Goolden</i>	51
17 On the Subcutaneous Injection of Quinine	<i>Surgeon-Major G. Y. Hunter</i>	55

DISEASES OF THE NERVOUS SYSTEM.

ARTICLE.	AUTHOR.	PAGE
18 On the Value of Electricity as a Therapeutic Agent for the Relief of Pain	<i>Dr. Leslie Jones</i>	58
19 On Pseudo-Hypertrophic Paralysis (of Duchenne)	<i>Dr. Wm. H. Barlow</i>	66
20 On Sleeplessness	<i>Dr. J. M. Fothergill</i>	72
21 On the appearance of Paralysis on the Side of a Lesion in the Brain	<i>Dr. C. E. Brown</i> <i>Sequard</i>	74
22 Two Cases of Neuralgia treated with Chloride of Ammonium... ..	<i>Dr. David Young</i>	83
23 Cases of Sciatica and Neuralgia successfully treated by Galvanism	<i>S. J. Knott, Esq.</i>	85
24 On Nitrite of Amyl in Nervous Cephalalgia ...	<i>Dr. R. A. Douglas-Lithgow</i>	87
25 On the Treatment of Sunstroke by the Subcutaneous Injection of Quinine	<i>Surgeon A. R. Hall</i>	88

DISEASES OF THE ORGANS OF CIRCULATION.

26 On Intermittency and Irregularity of the Pulse, and on Palpitation, Cardiac and Aortic ...	<i>Dr. G. W. Balfour</i>	91
27 Extreme Slowness, followed by Great Irregularity of the Pulse, with no Organic Disease of Heart, treated by Bromide of Potassium and Belladonna	<i>Dr. R. Somerville and</i> <i>Dr. Brunton.</i>	98

DISEASES OF THE ORGANS OF RESPIRATION.

28 On the Combined use of Morphia and Atropia in the Treatment of Spasmodic Asthma... ..	<i>Dr. G. Oliver</i>	102
29 How to Cure a Cold in the Head	<i>Dr. D. Ferrier</i>	104

DISEASES OF THE URINARY ORGANS.

30 On the Pathology and Treatment of Albuminuria	<i>Dr. T. L. Brunton</i>	10
31 Gallic Acid in the Treatment of Albuminuria ...	<i>Dr. J. T. Jamieson</i>	114
32 On a Case of Diabetes cured by the Skim-Milk Diet	<i>Dr. Scott Donkin</i>	115

SURGERY.

FRACTURES, DISLOCATIONS, AMPUTATIONS, AND DISEASES
OF BONES, JOINTS, &c.

ARTICLE.	AUTHOR.	PAGE.
33 On Excision of the Ankle	<i>James F. West, Esq.</i>	119
34 Demonstrations of Antiseptic Surgery, before Members of the British Medical Association in the Operating Theatre of the Royal Infirmary, Edinburgh, August, 1875	<i>Prof. Lister</i>	123
35 On Lister's Treatment of Wounds and Abscesses by the Antiseptic Method	<i>Thomas Smith, Esq.</i>	126
36 On the Antiseptic Treatment of Cases of Open Knee-Joint	<i>Dr. H. C. Cameron</i>	129
37 On Non-Union of Fractures	<i>H. O. Thomas, Esq.</i>	133
38 New Operation for Ununited Fractures	<i>Dr. Matthew Hill</i>	136
39 Shortening and Deformity of the Femur, conse- quent upon Fracture, Successfully Treated by Re-Fracture.—Dr. Cruise's Splint	<i>P. J. Hayes, Esq.</i>	140
40 Cases of Excision of the Knee-Joint	<i>P. J. Hayes, Esq.</i>	144
41 The Carbolised Catgut Ligature	<i>Editor of the Medical Times and Gazette</i>	149
42 On the Employment of Carbolised Catgut as a Ligature in Amputations and other Major Amputations	<i>Dr. A. W. Nankivell</i>	151
43 On Aquapuncture	<i>R. Clement Lucas, Esq.</i>	153
44 On the Electrolytic Dispersion of Tumours	<i>Dr. Julius Althaus</i>	158

ORGANS OF CIRCULATION.

45 Case of Aortic Aneurism Successfully Treated by the Distal Ligature	<i>Thos. Annandale Esq.</i>	163
46 On a Recent Case of Ligature of the External Iliac Artery by an Antiseptic Material, for the Cure of an Unusual Number of Aneurisms	<i>Oliver Pemberton, Esq.</i>	165
47 On Torsion of Arteries	<i>Dr. Robert M'Donnell</i>	166
48 On the Treatment of Nævus	<i>Dr. John Duncan</i>	170

ALIMENTARY CANAL.

49 On Cleft Palate	<i>Dr. E. A. Rawson</i>	177
50 On Hare-Lip and Cleft Palate	<i>Sir Wm. Fergusson, Bt.</i>	179
51 The Use of Liquor Bismuthi for Hemorrhoids and Prolapsus Ani	<i>Dr. John Cleland</i>	181

ORGANS OF URINE AND GENERATION.

ARTICLE.	AUTHOR.	PAGE.
52 On the Treatment of Urethral Disease	<i>Sir H. Thompson, Bt.</i>	183
53 On the Diagnosis of Diseases of the Urinary Organs	<i>Sir H. Thompson, Bt.</i>	185
54 On Physical Examination of the Urethra in Cases of Stricture	<i>Sir H. Thompson, Bt.</i>	186
55 On the best manner of Employing Instruments in Cases of Stricture where much Difficulty exists	<i>Sir H. Thompson, Bt.</i>	189
56 On Internal Urethrotomy, and on the Instruments employed for Performing it	<i>Sir H. Thompson, Bt.</i>	193
57 On some Important Points in connection with Lithotrity	<i>Sir H. Thompson, Bt.</i>	196
58 On the Retention of Bougies instead of Cathe- ters for the Continuous Dilatation of Strictures	<i>Furneaux Jordan, Esq.</i>	201
59 Salt & Son's Improved Catheter for Bed-ridden Patients		202
60 On Puncture of the Bladder <i>per Rectum</i>	<i>Richard Davy, Esq.</i>	203
61 On the Treatment of Acute Orchitis by Puncture of the Testis	<i>Henry Smith, Esq.</i>	205
62 On Puncture of the Testis in Acute Orchitis ...	<i>T. W. Nunn, Esq.</i>	206
63 On the Treatment of Incipient Stricture by Otis's Operation	<i>Berkeley Hill, Esq.</i>	207
64 On the Extension of Inflammation from the Epididymis to the Urethra	<i>Furneaux Jordan, Esq.</i>	213
65 On the Treatment of Vesical Irritability and In- continence of Urine in the Female by Dilata- tion of the Neck of the Bladder	<i>T. Pridgin Teale, Esq.</i>	216
66 On Dilatation of the Female Urethra	<i>Christopher Heath, Esq.</i>	218
67 Irritability of the Female Bladder of Fifteen Years' Standing cured by Dilatation of the Urethra and Neck of the Bladder	<i>H. B. Hewetson, Esq.</i>	219
68 On Dilatation of the Female Urethra	<i>Dr. A. W. Edis</i>	222
69 On the Rapid Dilatation of the Urethra in Women	<i>Prof. Spiegelberg</i>	225

AFFECTIONS OF THE EYE AND EAR.

70 Clinical Lecture on a Case of Cataract Extraction	<i>Dr. C. B. Taylor</i>	226
71 Section of the Cornea in Certain Diseases of the Eye of Inflammatory Origin: Abscess of the Cornea: Ophthalmitis: Irido-Choroiditis Sym- pathetic (?): with a Note on the Treatment of Hypopion by Ciliary Incision,	<i>J. V. Solomon, Esq.</i>	230
72 Examination of the Eyeball by Lateral or Ob- lique Illumination—Ophthalmic-Microscope ...	<i>Haynes Walton, Esq.</i>	234
73 On Conjunctival Transplantation from the Rab- bit to the Human Subject	<i>Dr. J. R. Wolfe</i>	236
74 On a New Method of making Pervious the Eus- tachian Tube and of Inflating the Tympanum	<i>Prof. Gruber</i>	240

AFFECTIONS OF THE SKIN.

ARTICLE.	AUTHOR.	PAGE.
75 On the Treatment of Chronic Eczema by Glycerole of Subacetate of Lead	<i>Dr. Balmanno Squire</i>	245
76 On Glycerole of Subacetate of Lead in Chronic Eczema	<i>Dr. J. H. Wathen</i>	251
77 On the Treatment of Scrofulides (Lupus)	<i>Dr. H. G. Piffard</i>	253
78 On the Treatment of Psoriasis by India-Rubber Underclothing	<i>Dr. Balmanno Squire</i>	258
79 On a more Effectual Method of Applying Iodine to the Interior of Certain Cysts	<i>Furneaux Jordan, Esq.</i>	259

SYPHILITIC DISEASES.

80 On Syphilis	<i>Dr. v. Sigmund</i>	260
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MIDWIFERY,

AND THE DISEASES OF WOMEN AND CHILDREN.

81 Salt & Son's New Speculum Vaginæ	265
82 On the Induction of Premature Labour	<i>Dr. Clement Godson</i>	266
83 On the Surgical Treatment of Prolapsus Vaginæ	<i>Dr. Thomas Savage</i>	271
84 On the Complete Evacuation of the Uterus after Abortion	<i>Prof. A. R. Simpson</i>	275
85 A Case of Difficult Labour from Abnormal Rigidity of the Os	<i>Dr. Geo. Calderwood</i>	279
86 Treatment of Shoulder Presentations	<i>Dr. Edward Warren</i>	281
87 Laceration of the Perineum	<i>Dr. James Young</i>	282
88 Report on the Treatment of Ruptured Perineum	<i>Thomas Smith, Esq.</i>	284
89 Folding Short Forceps... ..	<i>W. Draper, Esq.</i>	287
90 Against the Pendulum Movement in Working the Midwifery Forceps	<i>Dr. J. Matthews Duncan</i>	290
91 Remarks on the Inevitable and other Lacerations of the Orifice of the Vagina, and near it, in Primiparæ	<i>Dr. J. M. Duncan</i>	294
92 On Puerperal Fever	<i>Dr. W. O. Priestley</i>	300
93 On the Use of the Forceps at the Rotundo Lying-in Hospital, Dublin, during 1875	<i>Dr. G. Johnston</i>	311
94 Case of Artificial Occlusion of the Vagina for the cure of Prolapsus Uteri	<i>Dr. James Dunlop</i>	313
95 A New Method of Preventing the Secretion of Milk in the Female Breast	<i>Dr. J. W. Lane</i>	314
96 "No More Ovariectomy."	<i>Dr. Semeleder</i>	315
97 On the Use of Nitric Acid as a Caustic in Uterine Practice, and its Superiority as such to Nitrate of Silver	<i>Dr. Jas. Braithwaite</i>	316
98 On Mammary Tumours	<i>Dr. G. Buchanan</i>	318

ADDENDA.

ARTICLE.	AUTHOR.	PAGE.
99 An Official Multiple Antidote	<i>Dr. du Vivier</i>	327
100 Successful Employment of Phosphide of Zinc in a Case of Mercurial Tremor, and in one of Chronic Arsenical Intoxication	<i>Dr. N. G. de Mussy</i>	328
101 On the Administration of Alcohol in the Treat- ment of Disease	<i>Dr. B. W. Richardson</i>	330
102 On the Physiological Action of Alcohol ...	<i>Dr. T. L. Brunton</i>	335
103 New Method of Administering Ether Vapour ...	<i>Lawson Tait, Esq.</i>	341
104 Note on an Ether Inhaler	<i>Dr. Martin Oxley</i>	343
105 On the Advantages of Ether over Chloroform ...	<i>Dr. James Sawyer</i>	344
106 On the Administration of Anæsthetics	<i>J. T. Clover, Esq.</i>	347
107 Nelaton's Inversion Method in a Chloroform Accident	<i>Lawson Tait, Esq.</i>	348
108 On the Administration of Chloroform	<i>G. H. B. Macleod, Esq.</i>	349
109 On Railway Injuries	<i>J. E. Erichsen, Esq.</i>	355
110 On Professional Muscular Atrophy	<i>Dr. E. Onimus</i>	362
111 On the External Uses of Hydrate of Chloral ...	<i>Dr. William Craig</i>	363
112 On the Dangers of Breathing by the Mouth ...	<i>Dr. Guye</i>	366
113 Description of a Form of Stretcher	<i>C. R. B. Reetley, Esq.</i>	367

INDEX.

SYNOPSIS,

(ARRANGED ALPHABETICALLY), CONTAINING

A SHORT ABSTRACT OF THE MOST PRACTICAL ARTICLES IN THIS VOLUME, SHOWING AT A GLANCE, THE MOST IMPORTANT INDICATIONS OF TREATMENT PUBLISHED BY DIFFERENT WRITERS DURING THE HALF-YEAR.

AFFECTIONS OF THE SYSTEM GENERALLY.

DIPHTHERIA.—The particles which form the *materies morbi* of diphtheria implant themselves at first upon the mucous membrane of the throat, and there become developed into a dense fungoid growth. The disease is distinctly a local one at first, unattended by constitutional symptoms. These manifest themselves with a rapidity proportionate to the health of the patient at the time of attack. In the treatment our chief aim is to prevent the vital energies succumbing to the prostrating effect of the poison as it enters the system. The dietetic treatment must be simultaneous with local and general medical treatment; but the most important, in my opinion, is the local application of substances which destroy the poisonous properties of the deposit on the throat. This consists of carbolic and sulphurous acids along with the liquor ferri perchloridi. My application generally consists of carbolic acid, one part; sulphurous acid three parts; solution of perchloride of iron and glycerine, of each four parts. This is either applied with a large camel-hair pencil, or by means of the spray-apparatus, at intervals of two hours. The mouth should also be frequently rinsed out with a weak solution of Condyl's fluid in water, and the following mixture taken in dessert-spoonful doses every two hours:—*R.* Potassæ chloratis 3 iij; acidi sulphurosi 3 iijss; tincturæ ferri perchloridi 3 iij; glycerini ʒi; aquæ q. s. ad ʒvi. *M.* In this way, a medicament is applied to the throat every hour, and, to be successful in curing the disease, this energetic treatment is absolutely necessary. Of course, if the patient be sleeping, the usual rule must be observed; viz., never disturb a patient if asleep. (Dr. R. Bell, p. 47.)

The Pellicles of Diphtheria and Scarlatina contrasted.—The pellicle of diphtheria is a leathery membrane, tough and difficult to tear. It is not the product of "inflammation" in the

ordinary acceptation of that term. It has as its basis a peculiar fluid exuded from the surface on which it rests. This fluid, which may be described as cacoplastic lymph, agglutinates a profusion of epithelial cells. The membrane thus formed consists of compact tenacious layers, which give it a stratified appearance. The pellicle of scarlatinous sore throat, on the other hand, is pultaceous and easily torn. In cases of scarlatinous gangrene of the tonsils, the substance which simulates the false membrane of diphtheria will, on examination, be found to be sphacelated mucous membrane, with the addition of a pultaceous substance—a loose epithelial detritus. In a word, the important diagnostic distinction consists in the diphtheritic pellicle being a strong, tough membrane, and the scarlatinous simulation of it being a pultaceous, easily-torn stratum of detritus, or a portion of gangrenous mucous membrane. (Sir J. Rose Cormack, p. 44.)

FEVER.—It is established on grounds which do not admit of any question, that a fevered man or animal discharges more nitrogen than a healthy person or animal on the same nitrogen income; and, that, as regards man, the febrile excess amounts to something like three-quarters of the normal expenditure. We have also seen that in man there is during fever an excess of discharge of carbonic acid, and that this cannot be accounted for as the mere result of excessive respiration, but that no such excess is observed in the dog. We have now to bring these facts into relation with each other. In health, the whole of the nitrogen discharge is derived from food. In inanition, when nitrogen income vanishes, all the nitrogen which passes out as urea or otherwise is derived from stored or tissue albumen. In fever this is also the case, for the nitrogen income is as defective in the one condition as in the other; but besides the using up of stored albumen, there is an additional and altogether abnormal disintegration which, for reasons already stated, we believe to take place at the expense of blood corpuscles, of muscle or other tissue. That fact must be taken as a starting point in any attempt to understand the febrile process as a disorder of nutrition. (Dr. J. Burdon Sanderson, p. 17.)

Cold Water Treatment of Fever.—The application of cold water in fever cases seems to be more and more used. The matter itself is very simple. If a patient at 40° C. (104 F.) is placed in a bath at a lower temperature, he must quickly part with heat. In fever, the natural regulation of heat which keeps our body at an almost equal temperature is insufficient. The cool bath makes up for this. If we measure the temperature

after the bath, we shall find it lower than before. The blood that surrounds the cells of our nerve centres is less hot. The patient therefore feels stronger and quieter. Cold baths (15-20° C. = 60-68 F.) have the clearest effect. Extensive experience has taught that their action is most positive when they are short and often repeated. Very weak patients must begin with 35° (97 F.), and then the warmth must be lowered to 20° (68 F.), by carefully and gradually adding cold water. In the meantime the body should be gently rubbed. (Dr. C. Binz, p. 22.)

ENTERIC FEVER.—Dr. Klein has discovered the presence of minute vegetable organisms in cases of enteric fever. The facts which have been put together by the most able and experienced pathologists tend to show that the contagion of enteric fever is due to a specific and living organism which, when transmitted from a diseased to a healthy individual, produces the same disease in the latter; and, further, that the chief, if not the only, vehicles of poison are the ejecta of the bowels of an infected person. Contagion in the form of a living organism could not be expected to produce infection at once; and the theory that it is organised is *primâ facie* justified by the circumstance that a period elapses between the reception of the contagium and the manifestation of the disease, during which the poison apparently lies dormant, but is in reality ripening towards an active condition. According to Dr. Klein, no other view of the poison affords any explanation of the incubation-period. But hitherto no one has succeeded in pointing out any specific organic form as the probable cause of enteric fever, and hence the paper which Dr. Klein has prepared is one of the highest interest. (Dr. Klein, p. 9.)

MALARIOUS FEVERS.—*Subcutaneous Injection of Quinine.*—Quinine may be administered hypodermically in cases in which for any reason a rapid action is necessary, or in which the stomach is too irritable to bear its administration in the natural way. I determined to give subcutaneous injection of quinine a fair trial in the Bombay House of Correction, using six minims for a dose of the following preparation:—Citric acid, three hundred grains; quinine, eighty grains; distilled water, one ounce. To be dissolved with the aid of heat (six minims of this solution being equal to one grain of quinine). I think it may be conceded that the fever yielded as soon as when treated per os, if not sooner, the table showing that in most cases the attack was arrested on the second day, and in no case later than the fourth, notwithstanding the irregular mode of treatment as to time &c. consequent on

the patients being prisoners in a gaol. Beyond all question there was a great saving in the quantity used, the cases being conducted to a satisfactory conclusion, so far as fever was concerned, with a very moderate expenditure of quinine. This mode of treatment, however, on further trial, was found to occasionally produce sores, unless the cases were carefully selected in which it was used, and there were other disadvantages. My impression is that when the orthodox channel is closed in consequence of the intolerance of the stomach, or otherwise, and when the weakly state of the patient renders it not unlikely that the subcutaneous tissue may resent this short cut into the system at its expense, the wiser plan would be to exhibit the drug per anum; but the circuitous route per os, when practicable, is incomparably better and safer, and quicker too, should the short cut have the penalty of ulceration attached to it. (Surgeon-Major G. Y. Hunter, p. 55.)

RHEUMATIC FEVER.—*Salicylic Acid.*—For internal administration the acid must be prepared by repeated crystallisation, so as to be chemically pure. Its shining white acicular crystals have no smell, and dissolve completely in water and alcohol. This pure acid can be given internally in considerable doses without any of those unpleasant results which have followed the use of the commercial acid, which probably owes its caustic properties to the presence of other substances—for instance, carbolic acid. The pure acid only excites some dryness in the mucous membrane of the mouth and pharynx, followed by an increased secretion from their surfaces. This inconvenience can, however, be obviated by giving the acid in half-gramme or gramme doses every hour, in the form of powder, and enclosed in a capsule; and in the treatment of rheumatic fever the administration is continued until the joints which were previously affected can be moved without pain. To quote Dr. Stricker's words, "All the patients thus treated were not only relieved of their fever, but also of the local symptoms—*i.e.*, the swelling, redness, and especially the painfulness of their joints,—within forty-eight hours; most of them even within a much shorter period." (Ed. of Med. Times and Gazette, p. 33.)

Salicin.—In the absence of any remedy worth calling such for acute rheumatism, we recommend a trial of salicin. It long enjoyed a reputation for tonic and febrifuge properties. It has of late years however gone out of use. It produces no deleterious effects given internally in large doses, and in doses of twelve grains every three hours in a case of acute rheumatism it caused a fall of temperature (in the first case

related from 102·8 to 99·6), and abatement of pain and swelling. The salicin may be given simply in powder, mixed with a little cold water. The more acute the case, the more marked the benefit produced. In acute cases, its beneficial action is generally apparent within 24, always within 48, hours of its administration in sufficient dose. Given thus at the commencement of the attack, it seems sometimes to arrest the course of the malady as effectively as quinine cures an ague, or ipecacuanha a dysentery. (Dr. MacLagan, p. 34.)

SACCHARATED LIME AS A STOMACHIC IN TYPHUS FEVER AND OTHER COMPLAINTS.—The two points most necessary to insist on in the administration of this remedy are probably large doses and copious dilution. A teaspoonful of the pharmacopœial “liquor,” given after meals, is necessary, in most cases, to develop its effects as a stomachic, and should be diluted in three or four ounces of water; but in cases in which there is foulness of the tongue from acute causes, much the best plan is to fill a tumbler with water, introduce as much of the solution of lime as can be added without giving a very disagreeable taste, and let the patient drink of the mixture *ad libitum*; and when one tumbler is finished let another be filled. In fever it has no specific virtue, but in many instances I have had occasion to observe that when an hospital patient had a black, parched, and cracked tongue, like a cinder, with the teeth and lips black with sordes, saccharated lime being administered freely, the tongue was moist the following day, and in a few days the black crust had entirely loosened and disappeared. It is not suitable, however, when there is inflammation of the stomach, or great irritability indicated, for example, by central redness of the tongue, with sharp ragged edges. (Prof. Cleland, p. 16.)

SALICYLIC ACID AS AN ANTIPYRETIC.—Salicylic acid is a most valuable antipyretic when administered for this purpose. It is necessary that each dose should be a large one, as small or divided doses have little or no effect. Generally speaking, the minimum dose required to reduce the temperature effectually is 5·0 grammes (77 grains). Should such a dose have no effect, one may after four or five hours with safety give a second, or indeed even a third similar dose. I have seen cases in which 15 or 20 grammes of the salt have been given during twenty-four hours without any ill effect. The salt is readily soluble in water, and is taken by most patients without complaint. In those rare cases in which malaise or vomiting occurred three or four drops of chloroform sufficed to remove them. I have collected the results of one hundred

single doses in cases of typhoid of nearly equal severity. In order to test the value of the drug more accurately, it was always given at midday, before the usual afternoon rise of temperature. Almost immediately, the temperature began to fall, the maximum result being reached, in most cases, from four to five hours after the administration of the medicine, but in some cases not less than eighteen to twenty hours were required. The subsequent rise of temperature was as gradual as its fall, but in mild cases it never reached the same height as before. (Dr. C. A. Ewald, p. 31.)

AFFECTIONS OF THE NERVOUS SYSTEM.

DUCHENNE'S PSEUDO-HYPERTROPHIC PALSY.—This disease is a curious and probably comparatively rare one. Its pathology is not yet made out, but it is probably owing to some change in the vaso-motor nervous system. The characteristic symptom is enlargement or hypertrophy of various muscles of the body, so that the little patient looks like an infant Hercules. This is not accompanied by increase, but by lessening of the power of the affected muscle. The most common parts to be attacked are the muscles of the calves of the legs. Duchenne believes that the paralysis is due to a lesion of the vaso-motor system, and calls to witness the alterations of capillary circulation and temperature, though no lesion of the ganglionic system has been as yet demonstrated. As to the prognosis, the general opinion may be stated to be that in the first stage, before the muscles begin to enlarge, the prognosis, though grave, is not fatal; but that with the second period (that of enlargement) that hope leaves us; and still more is this so in the third or period of atrophy and debility. The treatment includes the usual hygienic measures—baths, frictions, and shampooing, the iodides of iron, of potassium, strychnia, or nux vomica, and where there are cerebral symptoms the bromide of potassium has been found very useful. Tonics are indicated, arsenic, cod-liver oil, and Parrish's chemical food (the compound syrups of phosphates), have also been used with benefit, and probably free phosphorus in some of its therapeutic forms might be of value. But of all means within our power, the most promising is the prolonged and careful use of electricity in the forms of the induced and the galvanic current. (Dr. W. H. Barlow, p. 66.)

ELECTRICITY AS A THERAPEUTIC AGENT.—The value of electricity for the relief of pain is hardly yet properly appreciated by the profession. Stöhrer's continuous current battery

with thirty cells is the one I use, and I have not yet met a case in which a greater number of cells was required: it may be well to add that the sponges supplied with the machine are far too small. I use whole Turkey sponges, from two to three inches in diameter, firmly tied over the ends of the ordinary electrodes; by this means, the sponges being well soaked in water, or salt and water, the burning sensation so much complained of, especially when the application is made to the rectum, is greatly reduced. In every case it is better to begin the application from a few cells only, and gradually increase the strength of the stream to the point required, as the greatest difference in the effects produced will be noticed in different individuals. In cases too where the sponges have to be applied to the head or spine the patient should constantly be asked if there is any dizziness, and if this supervenes the application should be immediately discontinued. Neuralgias of the face, even of one or two years' standing, may be relieved by electricity. Hardly anything relieves neuralgia of the face due to pregnancy, but it may not infrequently be relieved by two or three applications of the continuous current. The cure of sciatica is almost certain when thirty cells of Stöhrer's battery are used. (Dr. Leslie Jones, p. 58.)

HEMIPLEGIA ON THE SAME SIDE AS THE BRAIN-LESION.—

Although it is quite true that, as a rule, hemiplegia is on the opposite side of the body to the lesion of the brain producing it, yet there certainly are cases in which it has been on the same side as the brain disease. In a very large number of cases paralysis has appeared on the side of the encephalic lesion, when it existed in or on the pons Varolii, the cerebellum, the crus cerebelli, the medulla oblongata, or other parts of the base of the brain. Indeed, *in cases of disease of the petrous bone, or of the surface of the encephalon near that bone, paralysis, if it occurs at all, will appear as frequently on the side of the lesion as on the opposite side.* (Dr. Brown-Séquard, p. 74.)

NERVOUS HEADACHE.—*Nitrite of Amyl.*—The use of nitrite of amyl in nervous headache was first suggested by the late Sir James Simpson. He stated that he found the inhalation of two drops of the nitrite give immediate relief, however severe the pain. The following is the best mode of administering it. Place two drops on the palm of the patient's hand, and quickly diffusing these with the finger over the palmar surface, tell her to cover her mouth and nose with her hand, and to inspire deeply and quietly. No time should be lost after the nitrite is dropped on the hand, as it evaporates rapidly.

The patient should be seated while inhaling, as the peculiar effects of the nitrite are produced almost instantaneously, and may occasionally alarm a very nervous or hysterical female. Fortunately these symptoms last a very short time—generally less than two or three minutes—and with their cessation the pain almost invariably ceases. Two drops may be given as a draught in water, instead of by inhalation, but the latter mode is much more satisfactory. (Dr. A. Douglas-Lithgow, p. 87.)

NEURALGIA.—*Chloride of Ammonium*.—Although it seems difficult to say in what class of cases precisely chloride of ammonium is likely to prove of use, it is well worth a trial in obstinate cases which have resisted other treatment. Dr. Young (of Florence) relates a case of obstinate neuralgia of the ovary in which 15 grains of the chloride was given every two hours till three doses were taken. This, along with a pessary containing bromide and belladonna gave complete relief at the period of greatest intensity of pain. The next access of pain was successfully treated in the same manner. This time the cure remained permanent. (Dr. D. Young, p. 83.)

PAIN.—*Subcutaneous Injection of Water*.—It is a curious fact that subcutaneous injections of water often relieve pain just as effectually as when morphia is used. Nerve pains, neuralgia, sciatica, and the like are more likely to be relieved than the sufferings incident to chronic progressive diseases, as cancer. As to the manner of injection and quantity of fluid to be introduced, Dr. Dieulafoy recommends that the seat of greatest pain should be searched for, and eight or ten drops of water be there injected. This, he says, may be repeated ten, twelve, or fifteen times at one sitting, if necessary. Dr. Lafitte, in a letter to l'Union Médicale of Oct. 5th, 1875, says that he injects two grammes and then waits a couple of minutes, and if the pain be not relieved he then injects two grammes more. Even five or six syringefuls may be sometimes injected. He thinks that less than two grammes or more than ten should never be injected. (Mr. R. Clement Lucas, p. 153.)

I have attended many patients who had been in the habit of taking morphia, and have treated them with the hot water plan, to the great improvement of their health, and then weaning them of the baneful poison. Even in cases of delirium tremens which had been accustomed to the injections, and in the fury of delirium persisted in calling out for them, I have employed the hot water and so lulled them to rest. But here I would pause to admit that sometimes this plan

would fail me, and then I would fall back upon the injecting a small dose of morphia first, following it up with the use of the hot water in two, three, or four hours. But it is in the cure of sciatica and deep-seated pain, as well as in brow-ague and similar distress, that I have found the hot-water injections most beneficial; indeed, I cannot recall an instance of failure. For the purpose of carrying out this method of cure, I had, in 1868, a hypodermic syringe made after my own plan. The needle is some inches in length; the cylinder is silver (I had broken many glass tubes or cylinders when drawing in the hot water), and contains a drachm and a half of fluid, if such an amount should be required to be thrown in. The piston has not a screw action, but runs up and down like an ordinary syringe, in order that the water may be expelled with the amount of force the operator may think best. In some instances, burying the needle up to the very end, I force in the hot water amongst the muscles, and then withdraw the instrument immediately afterwards, placing the tip of my finger on the perforation point, and, by a rolling movement, dispersing the fluid in its bed. The long nozzle of the syringe acts as an acupuncture needle; the fluid as an additional counter-irritant; the patient complains of a smarting, burning sensation, soon followed by relief from the pain; and, if the operation be done in the patient's own home, and while he is in bed, sleep will often supervene. In this manner, I have frequently stopped sciatica, the agony of acute lumbago, or that resulting from a sudden strain or fall, besides other minor pains. (Dr. G. de Gorrequer Griffith, *Brit. Med. Journ.*, Dec. 4, p. 702.)

PHOSPHIDE OF ZINC IN SOME DISEASES OF THE NERVOUS SYSTEM.—Phosphorus has taken, during the last few years, a most important place in therapeutics, both as modifying the nervous system and improving its impaired nutrition. About eight or ten years ago, M. Vigier, a very learned chemist of Paris, pointed out the advantages of phosphide of zinc. This preparation is much more stable, more definite, and more easily handled than phosphoric oil, which is commonly employed. Whilst the phosphoric oil is very easily altered by oxidation, and untrustworthy in its effects, no variation of power, no change of composition takes place in the phosphide of zinc. Four milligrammes of this substance contain one milligramme of phosphorus, but in reality, according to the researches of M. Vigier, the physiological action is equivalent to only half a milligramme. I always prescribe two pills of four milligrammes each to begin with, and proceed gradually to six pills when the physiological effects, which

must be carefully observed, do not seem to be sufficiently marked. If any signs of intolerance appear, I suspend the course for some days, to begin again afterwards in the same way. Generally, after twelve or fifteen days I allow the patient to take rest, and discontinue the medicine during four or five days. (Dr. de Mussy, p. 328.)

SCIATICA AND NEURALGIA.—*Galvanism*.—It depends on the cause of the sciatica or the neuralgia how the galvanism is used, and if the cause is decided on, then the amount of galvanism applied would be, up to a certain amount, as a tonic and stimulant to a nerve which is asthenic; the cells used would be few, and the application of light. But if the complaint has been caused by cold, sitting on a wet seat, gout, &c., and the nerve is tender when pressed upon, and the pain very acute, then the cells must be very much increased, and the poles pressed well into the part. The operation should be only for a few minutes, but severe. Sometimes around the cutaneous parts on which the charcoal points have been placed there occurs a herpetic eruption. If galvanism is applied in this case it is the most powerful counter-irritant possible, and the relief to the sufferer is very great. (Mr. S. J. Knott, p. 85.)

SLEEPLESSNESS.—*Hypnotics in*.—The use of *opium* is rather indicated in conditions of insomnia which take their origin in pain. When there is vascular excitement present, it is desirable to combine with it direct depressants of the circulation, as aconite or antimony. The subsequent cerebral anæmia induced by the resort to opium is not so pronounced as is that induced by chloral. *Hydrate of chloral* is comparatively valueless in sleeplessness due to pain, and is inferior, in this respect, it is said, to the croton-chloral-hydrate. It is, however, very useful in conditions of vascular excitement, either alone, or in combination with opium. In the delirium of acute pyrexia in children it may be usefully combined with the bromide of potassium. In cases of sleeplessness where there is a sustained high blood pressure, or where there is distinct pyrexia, chloral hydrate is the hypnotic *par excellence*. It is, however, decidedly to be avoided in cases where the inability to sleep is due to worry and to brain exhaustion. The special use of *bromide of potassium* lies in its utility, where cerebral activity is kept up by far away peripheral irritation, especially when that irritation lies in the pelvic viscera. It may be given alone, or with opium, or with chloral, according to circumstances. (Dr. J. Milner Fothergill, p. 72.)

SUNSTROKE.—*Subcutaneous Injection of Quinine*.—If there is anything in the practice of medicine which may be described

as *magical*, it is the effect of the subcutaneous injection of quinine in sunstroke. A soldier was completely comatose from sunstroke, with dilated pupils, stertorous breathing, face flushed, skin burning hot, pulse full and rapid. A solution of 5 grains of quinine in 5 minims of dilute sulphuric acid, and 50 minims of water, was put under his skin, in different places, about the shoulders. Within one hour, the heat of surface had perceptibly decreased; he steadily improved during the night, was quite sensible next morning, and recovered without any bad symptoms. As far as I am aware, this was the first case in which quinine was *hypodermically* employed. (Surgeon A. R. Hall, p. 88.)

AFFECTIONS OF THE CIRCULATORY SYSTEM.

ANTISEPTIC LIGATURES IN LIGATION OF LARGE VESSELS.—

The carbolised catgut ligature may be used with complete success for tying the external iliac artery. This is proved by a case in which the catgut was applied close to aneurismal sac under Poupart's ligament. The wound was treated antiseptically and did admirably. The advantage is the harmlessness of the material left in the wound, enabling us to close it at once. (Mr. O. Pemberton, p. 165.)

AORTIC ANEURISM.—*Distal Ligature.*—From my experience in cases of thoracic and cervical aneurism, and from an observation of cases treated by my friend and colleague Dr. G. Balfour, I can testify to the value of iodide of potassium in relieving the symptoms and promoting coagulation in this disease; but I am inclined to express the opinion that, should a fair trial of this drug fail to give complete relief to the symptoms, the distal ligature of the carotid should be employed, provided the case be a suitable one for the operation. The test of the suitability of the case for operation being the effect which temporary pressure on the distal portion of the carotid has on the aneurismal pulsation. If such pressure diminish the aneurismal pulsation, and should there be sufficient space to ligature the common carotid above the tumour, then I think the case is a proper one for operative interference. The case reported further illustrates the value of the antiseptic catgut ligature and other antiseptic precautions, the combined use of which has, in my opinion, removed all the most serious risks in connection with the ligature of arteries. (Mr. T. Annandale, p. 163.)

EPISTAXIS.—In the case of a woman suffering from cardiac disease, in whom epistaxis had been going on some hours, I injected up both nostrils a solution containing one part of

liquor ferri perchloridi fortior and three parts of water with a simple glass syringe, with the result of immediately sealing up the nostrils and stopping the hemorrhage. A quantity of blood had escaped through the posterior nares, and this also stopped at once. I used about three ounces of the solution altogether. There was not the slightest return of hemorrhage, and in two days I removed the plug, which was as perfect as one made with lint or aught else. The second case, of a child, was also one of great severity, and all usual remedies had failed. Half an ounce of the solution (1 to 4) immediately checked the bleeding. I think the injection preferable to making a plug with lint soaked in a solution of iron, for two reasons: 1. It is much more easily applied; 2. It is more likely to reach the seat of hemorrhage, and consequently would be more effectual. (Mr. W. Cox, *Winchcombe*, *British Medical Journal*, Feb. 5, p. 157.)

HEMORRHAGE.—*How to Stop Hemorrhage from a Large Vein Accidentally Wounded.*—During an operation for cancer of the breast, in which it was necessary to remove some scirrroid glands from the axilla, an aperture was accidentally made in the axillary vein in one side. It was most undesirable to obstruct the main vein of the limb by tying it across like an artery, and to have introduced a pad of lint into the wound, to compress the orifice, would have been very unsatisfactory practice. Prof. Lister therefore put in practice a method he had long contemplated in case of such a circumstance happening. “All flow of blood being temporarily stopped, by pressure on the vein to the distal side, he threaded a fine sewing needle with the finest catgut antiseptically prepared, and passed it through the coats of the vessel at opposite points of the wound, and at a short distance from its edges, and then cutting off the needle, so as to leave two threads in its track, tied one thread round each half of the wound. The purchase thus secured upon the venous texture prevented the ligature from slipping, and the bleeding was permanently arrested.” (Prof. Lister, *Edin. Med. Jour.*, Dec. p. 485.)

NÆVUS.—Nævi about the size of a pin’s head, and seated on the skin, are not uncommon on the face. They sometimes spread, they are very enduring, and it is therefore well to destroy them while yet small. The best method is to puncture the little spot with a needle previously dipped in strong nitric acid. This is simple, certain, and leaves no visible mark. It may be destroyed with equal certainty by the heated needle, by the galvanic cautery, or by electrolysis; but these measures are inferior in simplicity and are prone to leave a more distinct cicatrix. (Dr. J. Duncan, p. 170.)

Destruction of Nævus by Electrolysis.—We can not only decompose a chemical substance by electrolysis, as is well known, but we can destroy an organic and living tissue with ease and certainty. An ordinary round flat nævus yields to one electrolytic application, while in extensive “port wine marks” half a dozen or more applications may be required. The current should be directed to the tumour by means of fixed rows of gold needles, connected with both poles of ten to fifteen cells of Becker-Muirhead’s battery. As soon as the connection has been made, a destruction of the texture of the nævus is seen to commence, the blood-vessels and the skin withering away rapidly under the eyes of the operator. The destruction is more thorough at the positive than at the negative pole, and the worst parts of the nævus should, therefore, always undergo the influence of the former. As a rule, not a drop of blood is lost during the operation; but if, by sudden movements of the child, one of the needles should come out prematurely and a drop of blood should appear, this can be immediately coagulated, and any hemorrhage checked, by applying the positive pole to the puncture. The whole mass of the tumour is then gradually brought under the influence of the current, care being taken not to act too long or too energetically upon any one point, as otherwise a scar might be left. When everything morbid appears to have been destroyed, the action is discontinued, and the surface covered with a piece of goldbeater’s skin. (Dr. J. Althaus, p. 158.)

AFFECTIONS OF THE RESPIRATORY SYSTEM.

COLD IN THE HEAD.—Being threatened with a cold in the head, sneezing, watering of the eyes, and considering that these symptoms were purely local, I thought it probable they would be relieved by some purely local remedy which would act as an astringent on the mucous membrane. Having some trisnitrate of bismuth at hand, I took repeated pinches of it in the form of snuff, inhaling it strongly, so as to carry it well into the interior of the nostrils. In a short time the tickling in the nostrils and sneezing ceased, and next morning all traces of coryza had completely disappeared. Bismuth alone, therefore, proved quite successful, but it is better in combination with the ingredients in the following formula. Bismuth by itself is rather heavy, and not easily inhaled, and it is, moreover, necessary that it should form a coating on the mucous membrane. It is, therefore, advisable to combine it with pulv. acaciæ, which renders the bulk larger and the powder more easily inhaled, while the secretion of the

nostrils causes the formation of an adherent mucilaginous coating, of itself a great sedative of an irritated surface. The sedative effect is greatly strengthened by the addition of a small quantity of hydrochlorate of morphia, which speedily allays the feeling of irritation, and aids in putting a stop to the reflex secretion of tears. The formula which I find on the whole the most suitable combination of the ingredients of the snuff is as follows: Hydrochlorate of morphia, two grains; acacia powder, two drachms; trisnitrate of bismuth, six drachms. As this is neither an errhine nor a sternutatory, but rather the opposite, it may be termed an anti-errhine or anti-sternutatory powder. Of this powder one-quarter to one-half may be taken as snuff in the course of the twenty-four hours. The inhalations ought to be commenced as soon as the symptoms of coryza begin to show themselves, and should be used frequently at first, so as to keep the interior of the nostrils constantly well coated. Each time the nostrils are cleared another pinch should be taken. It may be taken in the ordinary manner from between the thumb and fore-finger, but a much more efficacious and less wasteful method is to use a small gutter of paper, or a "snuff-spoon," placing it just within the nostril and sniffing up forcibly so as to carry it well within. Some of the snuff usually finds its way into the pharynx, and acts as a good topical application there, should there be also pharyngeal catarrh. The powder causes scarcely any perceptible sensation. A slight smarting may occur if the mucous membrane is much irritated and inflamed, but it rapidly disappears. After a few sniffs of the powder, a perceptible amelioration of the symptoms ensues, and in the course of a few hours, the powder being inhaled from time to time, all the symptoms may have entirely disappeared. (Dr. D. Ferrier, p. 104.)

SPASMODIC ASTHMA.—*Subcutaneous Injection of Morphia.*—An attack of spasmodic asthma may generally be relieved by a dose of morphia administered hypodermically. (Dr. W. McGregor Burns, Practitioner, Feb. p. 136.)

Subcutaneous Injection of Morphia and Atrophia Combined.—In a case of unusually severe midnight paroxysm of asthma, I injected a third of a grain of acetate of morphia; in about ten minutes the breathing was calm and nearly free from wheezing. On awaking next morning nausea, vomiting, and headache, detracted somewhat from the joy of my patient at having discovered a remedy so effectual and so speedy in its operation. After repeating the injection with the same results, I combined with the morphia $\frac{1}{100}$ gr. of sulphate of atropia. Within a shorter period—not exceeding five minutes

—the breathing was comfortable and free from wheezing ; and next morning nausea, vomiting, &c., were not complained of. On one or two occasions I used atropia alone ; the result being relief to the breathing in about ten minutes. It was quite clear the relief was much less decided than after morphia alone, and this was inferior to the combination of both remedies, which set the patient's breathing perfectly at ease for the night without the penalty of vomiting and headache in the morning. (Dr. G. Oliver, p. 102.)

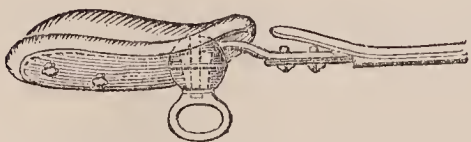
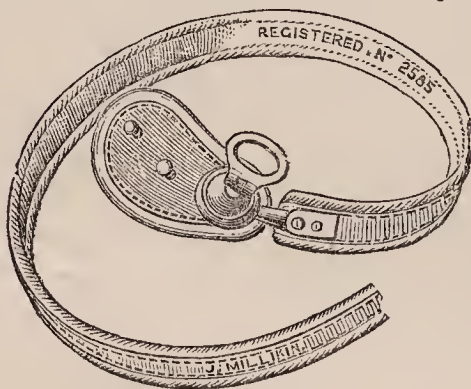
AFFECTIONS OF THE DIGESTIVE SYSTEM.

ALCOHOLIC DYSPEPSIA.—There are cases, commonly called cases of debility, in which there is no objective sign of organic disease. The leading symptoms are those of persistent dyspepsia ; flatulency ; irregular action of the bowels ; hemorrhoids ; much exhaustion under moderate physical exertion, and great mental depression under slight mental disturbances ; extreme nervous excitability, amounting to hysterical excitement ; a condition of urine variable in character, the fluid sometimes of straw colour, and abundant, at other times scanty and loaded with lithates ; the sleep disturbed, with frequent movements and muscular starts of the lower limbs at the moment of going to sleep ; a deficient appetite, and a white loaded tongue. In these examples—as common, by the way, to-day as in former times—the old practice used to consist in trying “to regulate” the wine or other alcoholic beverage. My experience now is that these symptoms are in nearly every instance caused by alcohol, and that the only certain successful treatment is total abstinence. (Dr. B. W. Richardson, p. 330.)

HEMORRHOIDS AND PROLAPSUS ANI.—*Injection of Liquor Bismuthi.*—A woman had prolapsus of the bowel to such an extent as to make it evident that at least half a yard of bowel was extruded. External support failed, and astringents had proved of no use. The patient was directed to mix a dessert-spoonful of liquor bismuthi with half a wine-glassful of starch, and, after getting into bed and returning the bowel to its place to introduce this enema and retain it. This treatment succeeded admirably—resulting in a complete cure. It was suggested by its known soothing influence in stomach affections attended with an irritable and congested mucous membrane. In severe hemorrhoids there are usually three parts affected, the integument, the mucous membrane, and the hemorrhoidal veins. Plainly the veins cannot be reached by local medicaments, and those comparatively few cases in which they alone are involved must be treated in other ways.

The integument, together with the edge of the mucous membrane up to the grasp of the sphincter, is within easy reach, and may be treated in various ways according to circumstances. Thus, when the congestion is superficial and produces a catarrhal oozing, bathing with whisky or other alcoholic lotion, a small pad of dry cotton-wool firmly applied to soak up the moisture, and also zinc or bismuth in powder or ointment are all exceedingly useful; and when a congested surface within easy reach is accompanied with venous engorgement, tincture of iodine sometimes produces surprising effects, although in other instances it is too painful to be borne. But when the mucous membrane is considerably involved, I know no application to compare with injection of liquor bismuthi, which has the advantage of being painless. (Dr. J. Cleland, p. 181.)

HERNIA.—*Improved Truss.*—Medical men are often troubled by patients with regard to their trusses, which they cannot fit so as to keep up the hernia. Upon examination it is frequently found that the pad simply requires to be turned a little to the right or left, or a little upwards or downwards, or inwards, as the case may be. The pad soon works back into



its old position, and the hernia passes as before, so that the truss has to be sent back to the instrument-maker with instructions to alter the direction of the pad. Mr. Millikin, of 35, St. Thomas's-street, Borough, S.E., has put before us a truss which seems to overcome many disadvantages. It will be seen from the diagram that the truss consists of the old German pad and spring, between which is placed a ball-and-socket joint, which can be fixed in any direction whatever, and also allows of great or little pressure, to

exercised at will. The fixing handle, *c*, folds down upon the truss, so that it does not show through the dress. The pad may be placed in any position. The extra weight and thickness are inconsiderable. The instrument is well made and is not expensive. (Lancet, Dec. 4, p. 807.)

OBSTINATE CONSTIPATION WITH SYMPTOMS OF OBSTRUCTION.—

A dose of morphia may be given hypodermically with great success in cases of obstinate constipation of the bowels, where the stomach rejects every form of purgative, and

where enemata utterly fail. After a hypodermic injection over the region of the stomach of fifteen to twenty minims of the ordinary solution of morphia, vomiting will cease, rest will be procured, and very soon the stomach will receive and retain castor oil or any ordinary purgative sufficient to move the bowels freely, and bring about the desired relief. (Dr. W. McGregor Burns, Practitioner, Feb., p. 186.)

STAPHYLORAPHY.—*A Substitute for Sutures.*—The insertion of sutures in this operation is both tedious and difficult. The following little contrivance, which can be obtained from Messrs. Weiss, is intended to facilitate the approximation of the edges of the cleft. “The instrument is simply a safety-pin, or rather needle, used on the same principle as that for fastening children’s clothes. It is about three-quarters of an inch long and slightly curved: at its head is a flat silver knob, to which is attached a spiral silver spring; this spring is shorter than the needle, and on stretching it a sheath at its free extremity receives the point of the needle. The edges of the cleft having been pared, the needle is passed in through one and out through the other, giving a good grip to secure apposition, and going nearly or quite through to the back; the silver sheath fastened to the spiral spring is then drawn across and receives the point of the needle, thus retaining the latter in its place. Two, or as many more needles as are required can be applied, and when union is complete, can be withdrawn by pulling the sheath from the point of the needle and taking the latter out in the usual manner. (Dr. E. A. Rawson, p. 177.)

AFFECTIONS OF THE URINARY SYSTEM.

ACUTE ORCHITIS.—There is no plan of treating acute orchitis so satisfactory as puncture of the testicle by a single incision, from half to three-quarters of an inch in depth. It is only in cases of the most acute form of the disease that I recommend this treatment, and in by far the majority of these instances I have found that the relief given to the pain in the testicle itself is very marked, and almost instantaneous; in fact, so much so that the patients who come to the hospital in severe agony are enabled to walk out of the building comparatively free from pain, and in many instances joking about the treatment to which they have been subjected. I have had patients return to me for similar treatment to the other testis, after undergoing the operation on the one at some previous time, and than this there can be no better proof of their estimation of the relief which is produced by the puncture. When questioned by pupils as to the *modus operandi*

of the remedy, I have answered that the sudden relief from acute pain is due to the circumstance that the unyielding tunica albuginea is freely incised, and that the tension is thus taken off the compressed and swollen testis; in some instances there is more or less serum effused into the cavity of the tunica vaginalis, and the withdrawal of this greatly assists in giving relief, but in many instances there is hardly any effusion of serum. (Mr. Henry Smith, p. 205.)

In a case of this nature, the testicle was not greatly swollen, but it was very hard; and the scrotum, which was thin, was unusually ruddy. I punctured the testicle, at two or three points on its anterior and outer aspect, with a bistoury one-tenth of an inch wide, to the depth of about four-tenths of an inch. The pain almost immediately began to subside, and convalescence commenced. I punctured the testicle, acting on the same principle that I should perform iridectomy in acute glaucoma. (Mr. T. W. Nunn, p. 206.)

ALBUMEN.—*Examination of the Urine for.*—It is a well-known fact, mentioned in most text-books, that with a small quantity of nitric acid, as with other acids both mineral and vegetable, albumen forms a compound which is not coagulable by heat, but readily coagulable by an excess of nitric acid. (Dr. George Johnson, British Med. Journal, Feb. 26, p. 256.)

The safest test for albumen is, I believe, in all cases the moderate addition of strong nitric acid in such a way as to form a distinct layer at the bottom of the test-tube. If it be added in very large amount, and uniformly distributed, resolution takes place; and, if added in a very dilute state and in small proportion, no coagulation occurs even when afterwards heated. A further addition of acid will, however, still throw down the albumen. (Dr. Charles A. Rayne, Pendlebury, Manchester. British Med. Journal, Feb. 26, 1876, p. 256.)

As the question of albumen or no albumen in urine appears to some a matter of doubtful definition, allow me to suggest the following practical points for observation. They are essentially practical; and let me add that, as clinical observers, our object should be not to problematise simplicity, which is often done, but rather read Nature as she is—simple, benevolent, and to the letter true. 1. Given a test-tube of perfectly clear urine, it matters not whether it be acid or alkaline, the application of cold nitric acid carefully introduced into the tube (that is by tilting the tube and allowing the acid to trickle gently down the side) will infallibly pro-

duce a cloudiness if albumen be present; this might be over the surface of the acid, at the bottom of the tube, or, at the expiration of twenty seconds, at the upper third of the tube. 2. Given a turbid urine of lithiasis, there is no more difficulty (if it be remembered that albumen separates at 170 degrees—that is, 42 degrees below boiling point). Let this urine be placed in a test-tube, and held over a spirit-lamp; the urates will dissolve as soon as the urine becomes warm; then, as it reaches 170 degrees, the albumen will be seen to separate at the upper third of the tube. Then with this be satisfied, and do not boil the urine; for if this be done, and the albumen be in small quantity, it will be re-dissolved. In order to test the accuracy of the experiment, filter away the urates, and test with cold nitric acid as before. 3. Given a semi-turbid, pellucid, pale urine, which one knows by experience to be phosphatic; and here, if the amount of albumen be slight, a little difficulty may be experienced; and the best mode of procedure, if accuracy be required, is to filter away the insoluble material, and to very carefully add cold nitric acid as before. It is a most unsatisfactory mode of procedure to boil the urine and at once to add the nitric acid; for by this oxidising process, new compounds are undoubtedly formed, and the albumen will be either re-dissolved, or, perhaps, chemically changed. Let me say that, if nitric acid be added to a cold clear solution of phosphatic urine, and it gradually become homogeneously pellucid, this is not due to albumen, but to saline matter. (Mr. Thos. S. Dowse, *British Med. Journal*, March 4, p. 286.)

ANÆSTHETICS IN OPERATIONS ON THE BLADDER.—I have never been very well disposed towards the employment of chloroform for any purpose when it could be dispensed with, but I have of late, mainly owing to the improved methods of producing anæsthesia, availed myself of it much more than formerly. I refer to the system which Mr. Clover always pursues, of administering first nitrous oxide for about thirty seconds, and then following with ether vapour. The rapidity and greater safety of the process as compared with that by chloroform, together with the freedom from subsequent sickness usually attained (the latter an advantage of no slight value), have influenced my practice, and I now make anæsthesia the rule and not the exception. (Sir H. Thompson, p. 198.)

CHRONIC CYSTITIS FOLLOWING LITHOTRITY.—I have only learned during the last few years how extremely small a quantity of urine habitually left behind in an elderly patient's bladder after each act of micturition, provided that

he is undergoing, or has just undergone, lithotrity, suffices to lead to phosphatic deposit and to chronic cystitis. You will scarcely believe me that one drachm, or one drachm and a half only of this residual urine is enough in most such cases to produce the condition so feared and detested by every lithotritist; and I now add, that if the condition is detected early, and this small quantity is promptly and frequently removed by the patient himself, which he can easily do with a soft *coudée* catheter, almost certainly the dreaded symptoms will not appear. For the purpose of washing out the bladder I advise the patient (who may or may not be habitually using the catheter to empty his bladder) to use every second or third day the following apparatus:—(1) A black flexible catheter, No. 11 or No. 12 in size, made thin, and with polished interior, so as to facilitate the passage of débris through it (a great improvement on the ordinary French flexible catheters, which are thick and have often rough interiors), and having a large oval eye on the upper surface of its extremity, which is slightly turned upwards (*coudée*). (2) An eight-ounce india-rubber bottle, with a brass nozzle which fits *over* the outer end of the catheter, and not into it. The manifest result of this mode of attachment is, that a powerful uninterrupted current can enter and issue from the bladder; indeed it is scarcely possible that débris should remain in the organ under the influence of the action of this apparatus, as any one can perceive on using it. It may also be used as an aspirator, with a backward and forward current, if desired. For the patient's own use it is even more easy than the ordinary four-ounce bottle, which I have long been in the habit of desiring such to apply, since the size and freedom of the channel—not narrowed at the point of contact between bottle and catheter—permit the fluid to be propelled with very slight pressure. An admirable method of treatment is the injection of a weak solution of nitrate of silver every day or every other day, for a short time only. Half a grain to four or six ounces of distilled water is strong enough. This injection is especially useful where there is a tendency to the formation of phosphatic calculus and chronic cystitis. (Sir H. Thompson, p. 199.)

HYDROCELE.—In boys and men there are occasionally encysted hydroceles of the testis, or the cord, which continue to increase in size, or in which treatment is urgently requested. In such cases, except in early infancy, acupuncture or the use of a fine trocar often fails to cure. The walls of the cysts are usually thin, and collapse so much when their contents are withdrawn that the injection of a fluid is uncer-

tain. The end of a canula may be outside the cyst, and the iodine solution be consequently injected into the connective tissue at its exterior. In such cases the following is a reliable method of treatment:—The cyst being well isolated, made tense, and brought near the surface, I pass through its centre a stout needle, armed with silk, and leave the threads hanging. The fluid quickly oozes away, especially if a little traction be made on the threads. I then, at one opening, wet the threads with iodine liniment (liniment because the quantity required is so limited) and draw the threads so as to leave moistened portions within the cyst. A little gentle friction will help to spread the iodine thoroughly over the lining membrane of the cavity. An hour later freshly moistened portions may again be drawn through if the cyst be large, or if other methods of treatment have failed. On the other hand, in a very small cyst a single thread, moistened and kept in one hour, will suffice. (Mr. F. Jordan, p. 259.)

INJECTIONS.—*To pass an Injection into the Urethra.*—For the purpose of passing an injection into the urethra you may be certain that a syringe containing one fluid drachm is amply sufficient, and that it will distend the urethra for three and a half or four inches. A half-drachm syringe often suffices. It is scarcely necessary to say that these small instruments are much more easily managed by a patient than larger and longer ones. But most patients, unless specially taught to use the syringe, never introduce any injection at all. Unless the orifice of the urethra is carefully closed at the time, the fluid simply leaves the end of the syringe and flows out by the external meatus; and in every case after the injection has been made, the moment the orifice is unclosed the fluid is rapidly expelled by the contractile force of the urethra, and no appreciable quantity remains within. (Sir H. Thompson, p. 184.)

OBSTINATE PERINEAL FISTULA.—*Puncture of the Bladder per Rectum.*—Obstinate perineal fistula not depending upon obstruction of the urethra are best treated by puncture of the bladder per rectum, and leaving there an elastic tube. By this means no urine enters the fistula, and either spontaneously or assisted by art it heals. The following is a new method of operating:—First introduce the staff into the bladder; turn the tip of the staff towards the base of the bladder and rectum; feel for the point with the finger in the rectum, and carefully cut the recto-vesical tissues until you can pass the staff out at the anus. Next unscrew the handle and affix the stem of one of Napier's retentive catheters to the thin end of the staff; soap well the india-rubber; then,

holding the silver tube firmly in one hand, draw the catheter (excepting its bell-shaped end) completely into the silver tube by withdrawing the staff at the anus. Lastly, draw back the silver tube and catheter through the urethra into the bladder; push in the catheter, and free the silver tube by also withdrawing it through the rectum out at the anus. The campanulate end of the catheter unfolds itself in the bladder, and its stem loosely hangs at the anus. (Mr. Richard Davy, p. 203.)

SCARLATINAL ALBUMINURIA.—*Gallic Acid*.—Give a teaspoonful of a saturated solution of gallic acid every two hours. It acts very decidedly in curing scarlatinal albuminuria, probably by a direct astringent effect upon the capillaries of the inflamed part. (Dr. J. T. Jamieson, p. 114.)

STRICTURE OF THE URETHRA.—*Physical Examination of the Urethra in Cases of Stricture*.—When a young man consults you for certain troubles relative to which you desire to learn whether urethral obstruction be a cause or not, do not be tempted for an instant to adopt so unnecessary a course (to say the least) as the introduction of very large instruments, or instruments with huge bulbs at the end of them. But simply take a flexible English gum-elastic bougie, well curved towards the point, with a blunt end (since a tapering point, of course, will not mark distinctly the site of stricture), not larger, as a rule, than No. 10 or 11 of our scale, and pass it very gently and slowly into the bladder. If it goes easily—above all, if it is withdrawn without being held, and slides out with perfect facility, take my word for it he has no stricture, and, *quoad* obstruction, wants no use of instruments whatever. In the case of an older man with confirmed stricture, the diagnosis is to be conducted with more attention to detail. I proceed as follows, making what may be called a “survey” of the passage. I commence by passing the soft, blunt-ended English gum-elastic bougie as far as it will go; and when this stops, as it may do within an inch or less of the orifice, I make a note of the distance, and next find what will pass with tolerable ease through the obstruction met with. Very likely a No. 4 or 5 (similar instrument to the preceding) will do so, and it may be then carried further to seek another check, which is not unfrequently met with at about five inches from the orifice. Through this, after a trial or two, a very small gum catheter—say No. 1 or 2—may probably be passed into the bladder, drawing off some urine, and so assuring you of its position. I know then the patient has, at all events, a narrowing near the orifice and another at the distance named. I may verify this at once with a

bulbous instrument if I choose; but as long as the anterior narrowing exists, it is as well to postpone more minute research until it is divided, as considerable narrowing at that part always requires to be. Such a one, indeed, is never advantageously treated by dilatation. This being done, the No. 11 blunt-ended bougie is passed, and we learn whether any contraction exists in the interval existing between it and the spot originally marked at five inches. If there is one, the stopping of the bougie will indicate its situation. But if the bougie passes easily to the point just named, the canal is sufficiently open, and we have arrived at what, in ninety-nine cases out of a hundred, is the only remaining stricture; for it is rare indeed, where a narrow stricture exists at that distance from the meatus, that any other will be found beyond. This has next to be dealt with, and, as we are speaking now of old and confirmed cases, there is little doubt that the best treatment will be internal urethrotomy. A word or two about the bulbous exploring instrument. It should, in my opinion, be of polished metal mounted on a slender metal rod or shaft. No other material slides so easily and smoothly through the urethra. (Sir H. Thompson, p. 186.)

Treatment of Difficult Cases of Stricture by Instruments.—First see the patient pass water in order that you may be certain that he actually has a stricture. Sometimes the difficulty which there may be in passing an instrument does not depend upon a tight stricture, but upon a false passage already made. In this case the stream would be pretty full. Estimate the size of the instruments you will require to use by the size of the stream of urine. If the urine only comes guttatim, after having ascertained the situation of the stricture, introduce the smallest English gum catheter without a stylet, and try to insinuate it through the stricture. For this purpose Messrs. Weiss have produced some exceedingly delicate instruments much smaller than No. 1, and containing a slender steel thread. If such an instrument can be passed, tie it in at once and leave it. You know that it is in the bladder because it is capable of transmitting urine in drops. This is far safer than a fine silver catheter—but this may be tried if the other fail—but it should only be tried by a hand well accustomed to use the catheter, otherwise a false passage is almost certain to be the result. The stricture may be more or less tortuous, in which case the instrument may be applied by what may be called “groping,” until by chance it has entered into the stricture, as you may feel by the point being held or grasped by it. Another plan is to examine the part systematically. We will suppose the urethra to require

examination on four sides—a roof, a floor, a right side, and a left side. You are to take your slender silver catheter and slide its point delicately along each side successively from the orifice to the place of obstruction. If the orifice of the stricture is not *exactly* in the middle line, and we may fairly believe that it rarely is so, then there is one side of the passage which will more easily lead into the narrow way than any other. If I go down on this side I shall probably not get into the stricture; but if I slide down gradually on the other I shall probably get the instrument in, because you see there is less obstruction on that side. Begin, then, by the roof. The roof is the firmest part, the least likely to furnish obstruction in the way of ducts or lacunæ, and by following it you will be most likely to carry the point in. The floor, on the contrary, is the softest, loosest, and most spongy part, and will be most likely to yield to the instrument and give way. If your first effort does not succeed, take the right side; if that does not do, take the left; if that fails try the floor. If you are very careful you may make the attempt in this manner for thirty or forty minutes without doing any damage; but if the patient suffers, or if he bleeds, or if you are losing patience, give up the task, or you may make a false passage, and increase the difficulty considerably. (Sir H. Thompson, p. 189.)

Management where there is a False Passage.—A false passage is nearly always on the floor of the urethra, because the mucous membrane is there looser, more yielding, and less supported externally, and it also contains more lacunæ. If you have passed an instrument into a false passage you may know at once by passing your finger into the rectum, when if so, there are only the coats of the bowel between your finger and the instrument, so that you feel it very distinctly. What you are to do, then, is to withdraw the catheter two inches or so, pass it on again, keeping as close along the upper part of the urethra as you can, ascertaining by means of the finger in the bowel that the instrument is not passing into the old route. But when a false passage exists, it sometimes constitutes the chief obstacle to the introduction of an instrument, for, as I have before hinted, the stricture itself may be only inconsiderable. The size of the stream will help to determine this point; and if, so far from being a mere thread, it has a volume equal to a No. 6 or 7 catheter, use a No. 5 silver one, so that you can guide it, and try each side of the urethra successively until you discover which it is that enables you to clear the orifice of the false passage. Remember the result, and you have a key to the difficulty which will enable you to overcome it with ease on the next occasion. (Sir H. Thompson, p. 193.)

Internal Urethrotomy.—It is a question of no mean importance which arises from the comparison of incision made by machinery and incision made by the intelligent hand. That is the real question in urethrotomy. And the instrument, which I always use, and have operated with so many times, is the type of the method which affords the opportunity of making the incision according to the operator's will and judgment. It is nothing more than a little knife with a long handle, and is used precisely as we use a scalpel anywhere else. Just as we should use a small knife in tenotomy without the sense of vision, where it is not necessary, but guided by the sense of touch, so do I advise you to cut in urethrotomy. I carry the parallel one step further, and remind you, that just as it is essential in tenotomy to divide completely the tendon, and also any little minor band of constricting tissue which opposes the perfectly free play of the joint, so is it essential to make the same complete division also in urethrotomy. The ultimate success in either operation depends on attention to these particulars. The instrument to which I have been referring is the urethrotome of Civiale, made much smaller, as I prefer it to be, than is customary in Paris. The shaft of mine is about the size of No. 3 English scale, with an oblong bulb at the end not larger than No. 5 or No. 6. Within this is a little blade, which, by a clever contrivance, the operator can make to issue at will, as much or as little as he desires, and which he can also sheath instantly at his pleasure. The bulbous end gives him the power of exploring the urethra at the time of the operation, and of determining precisely where and what he ought to divide. The bulb is to be introduced, say fully half or three-quarters of an inch beyond the maximum point of narrowing felt, the blade is then to be projected there, and drawn steadily through the whole of the constricted portion outwards towards the meatus, in which direction the incision is sure to be certain and complete. (Sir H. Thompson, p. 193.)

1 *Point in the Treatment of Stricture.*—In a large number of tight—perhaps very tight—old and neglected strictures, with no great irritability or resiliency or attendant induration, the surgeon is glad to get in a fine instrument and leave it in. Although it may require much patience and skill to introduce the finest bougie, the general custom is to withdraw it and attempt the introduction of a catheter. The effort occasionally fails, and much valuable time is lost. Now, in such cases I have for several years kept in the fine or filiform bougie, and directed that the urine should be passed from time to time by its side or around it. In every instance in a large number of cases this has been easily done, micturition, as a rule, actually

being easier and in a larger stream than when the bougie is out. In twenty-four hours a goodly-sized bougie (not catheter) can be introduced. On the third day I usually put in a No. 12 or even a No. 13 bougie or catheter. It is of course in the earliest stages that the superiority of the bougie over the catheter is seen. (Mr. F. Jordan, p. 201.)

URETHRAL DISEASE FROM INFLAMMATION OF THE EPIDIDY-
MIS.—Urethral disease following and resulting from inflammation of the epididymis is certainly rare, but when it does occur, is not recognised. Primary inflammation of the epididymis resulting from injury may be followed by urethritis with a creamy discharge, generally whiter than gonorrhœal discharge, but very likely, indeed almost certain, to be mistaken for it. Epididymitis may be a secondary affection to inflammation of the prostatic urethra, which, in its turn, may have been originated by extension of inflammation from the bladder. Many an obscure case of gleet can be explained in this manner. (Mr. Furneaux Jordan, p. 213.)

AFFECTIONS OF THE BONES, JOINTS, &c.

ANTISEPTIC LIGATURES.—*Preparation of Catgut for.*—I described the mode of preparation in the *Lancet* (April 3, 1869), as steeping the catgut in a mixture of carbolic acid and oil. It so happened that the carbolic acid which I used was liquid carbolic acid, so called—that is to say, crystallized carbolic acid, liquefied by the addition of water. Now, this water makes all the difference in world. When oil is added to this liquid carbolic acid, a considerable portion of the water is deposited in the form of very fine particles, which are suspended in the oil; and it is this mixture—this emulsion, if we may so call it—of oil and water which causes the remarkable physical change in the animal tissue of which catgut is composed, that alone renders it fit for our objects. The tissue of the catgut in the ordinary condition is utterly unfit for surgical purposes; as slippery, when moistened, as a piece of intestine in the dead-house—when you tie it in a knot, it slips with the utmost ease. But after it has been steeping in the emulsion of carbolic acid, water, and oil, for a certain length of time, it undergoes a physical change, which I am quite at a loss to explain. As the tissue lies steeping in this mixture the first effect is to moisten it somewhat; then, as time passes, after about a week, you find that, instead of becoming softer, more swollen and more opaque, as you would expect, it is, on the contrary, growing less opaque and beginning to shrink; and in about three months, though

still softer than dry catgut, it is comparatively firm, and quite transparent. Now, if you take a fresh piece of dry catgut and put it into this same sample of the preparing liquid, you will find the second piece become in the first instance partially moistened like the first; a fact which renders it inexplicable to me, why the former piece should have undergone what looks like a partial drying. But whatever the explanation, the all-important fact is this, that after the catgut has been thus partially dried, so to speak, in this moist liquid, it is now no longer liable to be made slippery by being steeped in water or the animal juices at the temperature of the body: it is indeed rendered softer and somewhat opalescent, but a reef-knot tied upon it holds better than one on waxed silk. (Prof. Lister, p. 123.)

AMPUTATIONS AND MAJOR-AMPUTATIONS.—*Carbolised Catgut as a Ligature in.*—Carbolised catgut of sufficient strength forms a perfectly efficient ligature for arteries of the largest class, such as the femoral. Some care and judgment perhaps is required in applying it. It is found rather to prevent the occurrence of secondary hemorrhage, for out of over 60 major operations (including 22 amputations of the thigh and two excisions of the hip) secondary hemorrhage occurred only once, and this was not from failure of the ligature. The prepared catgut is sold in hanks steeped in the carbolised oil. It should always be kept in the liquid in stoppered bottles, and only taken out when required, for if this be not done, it is apt to become rough and brittle. When about to be used—as, for instance, in amputation of the thigh—an assistant should cut a piece of about four inches in length for the ligature of the main vessel from No. 4. The remaining arteries should be tied with No. 3. I find that about eight inches of the gut is enough to secure four vessels. The catgut should not be too old, for I have observed that some that I kept in stock for about two years became far more brittle than when more recently prepared. In tying vessels in an open wound I am very particular in separating the artery from its sheath and accompanying veins before proceeding to apply the ligature, and care should be taken to avoid all jerking in tightening the catgut. When the material is sound, it will bear steady pressure without breaking. I venture to claim the following advantages for the catgut—(1) immediate closure of the deeper parts of a wound; (2) absence of sinuous tracts; (3) diminution of subsequent bleeding, or rather its entire absence in consequence of the free use of ligatures to bleeding points during the operation; (4) absence of danger from accidental traction on a ligature; (5) above all, the almost entire avoidance of risk from second-

ary hemorrhage as illustrated by the results of the cases given above. (Dr. A. W. Nankivell, p. 151.)

EXCISION OF THE ANKLE.—Excision of the ankle is one of those operations in which the advantages afforded by Es-march's bandage are most appreciated. Not only is the patient free from hemorrhage, but in consequence of the parts not being covered with blood, we can see clearly what we are doing, and need only take away as much of either of the bones as is absolutely diseased, and if on taking one section of bone caries is still evident, we can take slice after slice of bone away until we get to sound osseous tissue, without having our view of the joint cavity in any way obstructed. The joint having been opened on the outer side and the fibula exposed, I separate the periosteum from it with the raspatory, and then cut off the lower end of that bone with the chain-saw. I prefer the chain-saw because it can readily be passed round the bone, and can be kept close to it, so as not to endanger the peronei tendons, and hence I think it presents greater advantages than Hey's or Butcher's saw. I then divide the internal lateral ligament, keeping close to the bone, so as to avoid the flexor tendons and the posterior tibial artery, push the lower end of the tibia through the fibular opening, denude the periosteum, and cut the articular surface and inner malleolus off with Butcher's saw, and lastly with Hey's saw I remove the upper part of the astragalus. Should that bone be extensively diseased it should be freely gouged, and the carious or necrosed portions cleared out with the osteotrite. (Mr. J. F. West, p. 119.)

NON-UNION OF FRACTURES.—There are many ways of treating an ununited fracture, but most involve some surgical proceeding. In a case of ununited fracture of the lower end of the tibia and fibula, as none of the usual methods could be applied in this case with any safety, I decided to try what effect a repeated application of percussion, with a rubber-protected mallet applied all round the articulation, would have. I accordingly commenced the treatment on the 20th of March, and applied percussion every second or third day for four weeks, desisting only for a few days when the parts became irritable. There was applied no splint or appliance whatever. From the first day of the operation there was a daily increase of consolidation and stiffness of the part, which continued until it resulted in a complete bony union in the space of four weeks; and on the 24th of May the patient was dismissed from the hospital quite cured, and free from this defect, the lower end of the tibia and fibula being considerably enlarged. In two other cases per-

cussion was only once performed. The essential point appears to be the production of some degree of periosteal irritation. Messrs. Khroné and Seseman, of London, supply a very neatly-constructed copper-headed mallet which I designed for these operations. (Mr. H. O. Thomas, p. 133.)

AFFECTIONS OF THE SKIN.

BRONCHOCELE.—Cystic bronchocele yields to a variety of operative procedures, but *Solid Bronchocele* is difficult to deal with, and the ablest surgeons of the present day habitually refuse to interfere in such cases, there being imminent risk to life attached to such operations as were previously practised. These tumours, however, often become dangerous to life by pressure on important neighbouring organs, and electrolysis then steps in as the right thing in the right place. I have of late years combined injections of the tincture or liniment of iodine into the substance of the tumour with the electrolytic applications, as it seemed to me that by such combined treatment time was gained. Iodine, subcutaneously injected, appears to break up the internal structure of the tumour, more especially where this is very old and tough, and aids electrolysis by allowing freer passage of the current through the mass. (Dr. J. Althaus, p. 160.)

CHRONIC ECZEMA.—*Lead v. Zinc Ointment.*—Lead is an infinitely more suitable agent in the local treatment of eczema than zinc. Lead, however, has been but little used in this country for the purpose. The agent that has been, and still is, chiefly in vogue for this purpose is zinc. It would not be untrue to assert that for some time past—how far back that time may reach I am unable to say—the two principal prescriptions for skin disease in this country have been zinc ointment and zinc lotion. As to the zinc ointment, it is a tradition that it must be *benzoated* zinc ointment. Lead however, is, I am well persuaded, by far a more soothing, and at the same time a much more astringent application than oxide of zinc. I have found that it unquestionably allays the itching, restrains the discharge, and diminishes the hyperæmia of eczema; in short, it *cures* it far more speedily than zinc does. It is of the best mode of applying this efficient agent in the treatment of chronic eczema that I have now more particularly to speak. As a lotion it fails, and I think it is not difficult to see how this happens. The lotion fails to keep the surface moist; the warmth of the body speedily causes it to evaporate: that is to say, the water passes off as vapour, and the lead is left on the sur-

face of the comparatively dry gummy layer of viscid exudation which is characteristic of eczema. Thus the remedy is precluded very considerably from influencing the skin itself, and is deposited simply on the surface of the scab, however minutely dotted and however thin that scab may be. Lead ointment is far more efficient. It preserves the moisture of the secretion, and so permits of the passage by osmosis of the remedy from the ointment through the secretion to the surface of the skin. Glycerine has proved in my hands far superior either to oil or water as a vehicle for applying remedies in the case of chronic eczema. It has neither, so to speak, the dessicating effect of water nor the macerating effect of grease. Its viscosity is about parallel with that of the viscid secretion proper to eczema, so that, in smearing it over, it is readily miscible with it, and while it preserves the surface moist, it readily incorporates itself, and more than that, undergoes true admixture or diffusion (osmosis) with the viscid aqueous solution of lymph and albumen which is exuded slowly but continuously from the surface of the diseased skin. Thus it conveys the remedy held in solution by it more freely to the actual surface of the skin itself. The first effect of the application, even in cases where, both as regards quality and strength, it may be excellently adapted to the condition present, will, to the unpractised eye, seem an aggravation of the disease—that is to say, the eczematous skin will seem to be redder and more inflamed, as well as moister. The moisture will, of course, be readily understood, but I find that the redness is very liable to misinterpretation, and so becomes the occasion of a loss of faith in the remedy at the very outset, and even the cause of it being promptly abandoned. It is due simply to greatly increased transparency of the cuticle, and of the thin scabs or scales, consequent on their infiltration with glycerine, which thus enables the inflamed true skin to shine through them somewhat more vividly. The skin, previous to each fresh application of the glycerole, ought to be boldly washed with a soft sponge, well moistened with warm soap-and-water. The remedy is manufactured—or, rather, I should say, the stock which forms the basis from which the remedy is prescribed is made—in precisely the same way as the “*plumbi subacetatis liquor*” of the British Pharmacopœia, only that glycerine is used in its manufacture instead of water; and to this I have to add that some simple directions as to temperature have also to be followed. It is thus prepared: Take of acetate of lead, 5; litharge, $3\frac{1}{2}$; glycerine, 20. Heat for half an hour in a boiling glycerine bath, constantly stirring, and filter in a gas-oven or other kind of heated

compartment. The result is a perfectly clear and colourless liquid, of a somewhat more viscid consistency than pure glycerine. Physically, it is a stronger solution than the Pharmacopœia "plumbi subacetatis liquor," inasmuch as the insoluble white residue left by the Pharmacopœial process is nearly doubled by weight the residue left by the process above directed. Therapeutically, however, the glycerine preparation is a far less active application considered absolutely than the "liquor," besides that it differs markedly in several essential respects in the *kind* of effect produced by it. It does not appear to be known that equal strengths of solution in water on the one hand, and in glycerine on the other, of any remedy used locally, are very different as to the degree of effect exerted on the skin, the aqueous solution being in every case a far stronger application than the glycerine solution. The reason of this is very obvious: the rapid evaporation of the water under the influence of the heat of the body very soon concentrates the aqueous solution; whereas the comparatively fixed character of glycerine does not allow of any notable evaporation of the solution. (Dr. B. Squire, p. 245.)

Dr. Wathen's method of making the preparation of lead is rather different from Dr. Squire's. It is composed of lard, glycerine, and the subacetate liquor, prepared in the following manner:—Into one ounce of best clarified lard two or three drachms of glycerine is rubbed. After this has been incorporated the addition of the liquor is commenced; the lead is rubbed in until the lard becomes saturated and will not receive any more. In the manner and care with which this is done consists the whole secret of the success of this preparation. If great care be taken to coax and tease the liquor plumbi in, as much as one ounce of the latter can be incorporated with the lard before it becomes saturated. Never has a single case occurred to him of the faintest suspicion of absorption of the lead into the system, although a considerable amount of surface has been enveloped in ointment—for instance, the whole of both legs, from knee down to the foot. This ointment has been found of great service in relieving the suffering in some forms of pruritus, particularly that of the genitals and anus. Herpes preputialis has also given way to this remedy. (Dr. J. H. Wathen, p. 251.)

LUPUS.—The tubercular form of lupus may be conveniently treated, as recommended by Hebra, by means of arsenical paste. The formula employed is: *Acidi arseniosi*, gr. xx; *cinnabaris*, dr. j.; *ungt. rosæ*, oz. j. M. The paste should be thinly spread upon linen, which is cut in narrow strips, and accurately applied to the affected part. It is left in position

for twenty-four hours, when a fresh application is made without previous washing. If there is deep ulceration nothing will succeed except excision, with subsequent cauterisation with a layer of deliquesced chloride of zinc. (Dr. H. G. Piffard, p. 253.)

PSORIASIS.—*India-Rubber Underclothing*.—In a case of obstinate or universal psoriasis the effect of retaining the natural moisture of the skin by wearing India-rubber underclothing may be tried. The patient should wash daily all over with warm soap and water. (Dr. B. Squire, p. 258.)

RINGWORM.—*Boracic Acid*.—Surgeon-Major Watson reports in the Indian Medical Gazette that he has lately employed boracic acid with very great success as an external application in the treatment of the dermatophyta, or vegetable parasitic diseases of the skin. He was induced to try this remedy from witnessing its employment as an antiseptic in the Edinburgh Infirmary wards. The diseases in which he has hitherto used boracic acid have been the different forms of tinea (*T. tonsurans* and *circinata*), and in that very troublesome form of the disease which affects the scrotum and inner side of the upper part of the thighs of many Europeans in India. Dr. Watson declares that the external application of a solution of boracic acid acts like a charm in such cases. An aqueous solution of boracic acid of a drachm to the ounce, or as much as the water will take up at ordinary temperatures, is employed. The affected parts should be well bathed with the solution twice daily, some little friction being used, and it should not be wiped off, but allowed to dry on the part. The remedy is said to be so simple, cheap, and efficacious, that it has only to be once used to be preferred to all other remedies of the same class. (Dr. Watson, *Lancet*, Nov. 20, p. 750.)

AFFECTIONS OF THE EYE AND EAR.

CATARACT EXTRACTION.—Having decided upon associating an iridectomy with the operation of extraction for cataract, the question arises, shall we do iridectomy now and extract later, or shall we remove the lens and a portion of iris at one and the same time? I should certainly much prefer to make the iridectomy a separate operation, and extract some weeks hence when all trace of the operation, such as haziness of the cornea at the site of the wound, or congestion of the neighbouring tissues, had passed away. The eye is always turned pretty firmly upwards during anæsthesia, and involuntarily, without anæsthetics, by the patient; to turn it downwards

with forceps in order to expose the upper segment of the cornea involves the use of a speculum, and is attended with some risk of rupture of the hyaloid membrane, and immediate loss of the eye. The upward tendency of the eyeball, and the downward gravitation of inflammatory products are, on the contrary, rather fortunate incidents if the lower section be adopted; and although I have operated successfully in a large number of cases by the upper section, I think there can be no question that the lens, the capsule, and remnants of cortex are much more easily extruded through an inferior incision. The next question which arises is, what anæsthetic to employ. Nitrous oxide is safe, and scarcely ever causes vomiting, but it occasions struggling, spasm, and a kind of epileptic condition, so that insensibility can seldom be conveniently maintained until the operation is completed: chloroform, bichloride of methylene, and methylene ether are pleasant to inhale, but they all cause vomiting—that from chloroform sometimes lasting for twenty-four hours, while the patient is with each of them exposed to the risk of fatal syncope, or death beginning at the heart. Ether is the only available anæsthetic. (Mr. C. B. Taylor, p. 226.)

EUSTACHIAN TUBE.—*New Method of Making Pervious, and Inflating.*—The method which has of late years been employed with much success for this purpose is that which was first described by A. Politzer. This, as is well known, is as follows: The patient having taken a small quantity of water in his mouth, the end of the nozzle-piece of one of the indiarubber bags, which Leiter (the instrument-maker) has specially constructed for the purpose, is passed about half an inch up the nose; the nostrils are next hermetically closed on the nozzle-piece by means of the thumb and forefinger of the operator's left hand, and then during the swallowing of the water, which the patient has to perform at the command of the operator, the latter, holding the bag in his right hand, suddenly compresses it. Prof. Gruber's method is founded upon the fact that during the pronunciation of the consonant *hek*, as in *hock*, strongly accentuating the *k*, the Eustachian tube is opened. It is better for the purpose to pronounce in order the words *hack*, *heck*, *hick*, *hock*, *huck*. In pronouncing these syllables in this order, we may convince ourselves that the tongue becomes drawn further backwards and more strongly upwards as we proceed with the list of syllables, so that with the syllable *huck* the tongue is most retracted, and that the upper pharynx is contracted and forcibly shut off. The operator stands, or sits at his convenience, face to face with the patient. The end of the nozzle-piece of a rubber bag, which the operator grasps in his hand, is intro-

duced into the inferior nasal meatus of the patient for about half an inch, and the operator at once hermetically closes the nostrils on the nozzle-piece of the bag with the fore and middle finger of his other hand, and while the patient at the word of command pronounces one of the before-named syllables (*hack, heck, hick, hock, huck, hck*) the bag is squeezed, Thereupon the air passes with a distinctly perceptible noise through the tube into the cavity of the tympanum. The noise which is produced by the forcible entry of the air through the tube can be auscultated with precision by means of the otoscope. In cases where the membrana tympani is perforated, the well-known perforation sound is produced. The patient feels a distinct sensation of the entry of the air into the tympanic cavity. (Prof. Gruber, p. 240.)

ILLUMINATION FOR EYE OPERATIONS.—The following method of illumination is exceedingly useful when operating in eye diseases. The light of an ordinary lamp is passed through a globe bottle filled with water and focussed on the spot we wish to light up. In this way we obtain a column of light, about three inches in diameter, of great brilliancy, quite equal to the best daylight, and far superior to the operating-lamps which are generally used for the same purpose, and which, in my experience, are apt to be misdirected by assistants, to explode, and to ignite the vapour of ether when that anæsthetic is used. In fact, the most delicate manipulations may be confidently undertaken at any hour with this simple apparatus, which is at hand in every house. (Dr. C. B. Taylor, *Med. Times and Gazette*, Feb. 12, p. 181.)

TRANSPLANTATION OF CONJUNCTIVA FROM THE RABBIT.—Symblepharon, or adhesion of the globe of the eye to the lid, has only been cured hitherto by transplantation of conjunctiva from the patient's own eye wherever it can be got. Sometimes, however, there is none to get, or so little as to be of no service. It is found, however, that conjunctival membrane can be successfully transplanted from the cornea of a rabbit. I generally put both the patient and the rabbit under chloroform. I then separate the adhesions so that the eyeball can move in every direction. Next I mark the boundary of the portion of the conjunctiva of the rabbit which I wish to transplant by inserting four black silk ligatures, which I secure with a knot, leaving the needles attached; these black ligatures indicate also the epithelial surface, which would be very difficult to distinguish after separation. I take from the rabbit that portion of the conjunctiva which lines the inner angle, covering the membrana nictitans, and extending as far as the cornea. I select this on

account of its vascularity and looseness. The ligatures being put on the stretch, I separate the conjunctiva to be removed with scissors, and I transfer it quickly to replace the conjunctiva palpebræ of the patient, securing it in its place by means of the same needles, and adding other two stitches, or more, if requisite. Both eyes are then covered with a bandage and dry lint. For the first forty-eight hours the conjunctiva has a greyish look, but it gradually loses that appearance, and, with the exception of some isolated patches here and there, it becomes glistening, in some parts looking not unlike conjunctival thickening. These patches gradually decrease until the whole assumes a red appearance. (Dr. J. R. Wolfe, p. 236.)

MIDWIFERY, &c.

ABORTION AND MISCARRIAGE.—*When these occur what is the best way of Emptying the Womb?*—1st. The uterus must be compelled to steady action by the administration of ergot, introduced in the form of a strong solution of ergotin in the subcutaneous cellular tissue—a mode of administration of the drug which is destined, I believe, to supersede those with which we have so long been familiar; for the subcutaneous injection of ergotin sets up the uterine contractions with such speed, such certainty, and such safety, that the hypodermic syringe will be found an indispensable part of the furnishing of every obstetric bag. (I use for hypodermic injection a solution of one part of ergotin in three of water, with ten or twelve grains of chloral in each half-ounce bottle of the mixture. Ten drops of this fluid gives the action of the drug with great certainty, and I have seen no ill effects from it.) 2nd. Pending the onset of effective pains, and with the view of at once hastening them and preventing the escape of the blood, we plug the genital canal. For the most part, and as if it were the best in all cases, authors recommended the use of the vaginal tampon. Now, the plugging of the vagina, by whatever material, is sometimes the only expedient to which we can have recourse, and we may rely on it with much certainty for adverting the immediate danger of excessive hemorrhage. But it seems to me that, where we have our choice, its application ought to be restricted to those cases where there is still some hope that, if we succeed in arresting the hemorrhage, we shall at the same time succeed in averting the impending abortion. Otherwise we have a means of controlling the bleeding more directly, at once less

irksome to the patient, and less troublesome of application by the practitioner, in the cervical plug. For this purpose there is nothing so efficient as a good-sized sponge-tent, prepared, as Messrs. Duncan, Flockhart, and Co. now always prepare them, by being dipped in a disinfectant solution before being compressed by the whipcord. In my judgment, nothing can take the place of the sponge-tent for rapid, kindly, and complete dilatation of the cervical canal. (Professor A. R. Simpson, p. 275.)

INDUCTION OF PREMATURE LABOUR.—The old method of puncturing the membranes for the induction of premature labour is to be condemned. Inserting a long gum-elastic catheter between the membranes and the wall of the uterus, and leaving it there, coiling up the end in the vagina, has been found very effectual, but certain objections have been raised to it. It is said that the placenta *may* be partially separated by it, and the membranes *may* be accidentally punctured while pressing it. These should be improbable consequences, and I believe it to be one of the best methods which we have; for, should it fail, it may be followed up by the insertion of sponge-tents. In America, the term “colpeurysis” is used to signify the dilatation of the vagina, and a vulcanised gum-elastic bag called Braun’s colpeurynter has been employed to dilate the vagina, with the view of setting up uterine action. While stretching the vagina, it is supposed to act also upon the os uteri, and Dr. Meigs says it is found that about four hours suffice to dilate the os uteri sufficiently to provoke good labour pains. I do not know that it has ever been carried into practice in this country. Dr. Barnes puts in an elastic bougie over-night, which, he says, gives rise to uterine action; next day he introduces his bags. Uterine action has been set up by the bougie, in other words, labour has commenced; why then interfere with a process which, started artificially, may be completed gradually by the efforts of nature? If this argument of carrying out the process of labour “at one sitting, as one would cut for stone,” applies in premature labour, why should it not in labour at full term? If not likely to be followed by evil consequences, it would be a great boon to the medical attendant. To be kept about by a patient whom one has found in the first stage of labour is extremely trying. How much time and annoyance would be saved if we could proceed at once to “carry out at the one sitting” the delivery! And I cannot see why it should apply in one case more than in the other. Barnes’ bags are of the utmost value when it is desirable to complete the labour in as short a time as possible—for instance, in puerperal convulsions; but in induction of premature labour,

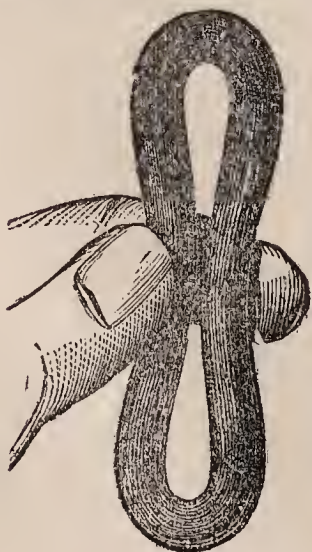
for causes not requiring hurry, to my mind their employment partakes too much of an operation, when there is no necessity for such discomfort to the patient. My mode of procedure consists in insinuating, night and morning, between the cervix uteri and the membranes, sponge tents of gradually increasing size; the first, and each succeeding one, being as large as the parts will admit. On removing each tent, and before replacing it by another, a warm douche, containing Condyl's fluid, is administered. I have found the use of one, two, and three tents to be sufficient, and have never had occasion to employ more than four. The instrument by means of which the tent is placed in position is made for me by Messrs. Arnold, of West Smithfield. (See Fig. p. 270.) It entirely obviates the use of the speculum, and being provided with what is equivalent to a universal joint, it enables the tent to be pushed, without extraneous guidance, between the cervix and the membranes, taking of itself the readiest path presented to it. For the same reason the membranes run no risk of puncture. The tents themselves are short, rounded at the extremity, and perforated, to facilitate adaptation to the instrument. The apparatus, and the mode of its application, are so simple, and so free from inconvenience and danger, that its use causes in practice little or no anxiety on the part of the patient; and until labour sets in, she moves about without pain or inconvenience, regardless of the presence of the tent. (Dr. Clement Godson, p. 266.)

INEVITABLE LACERATION OF THE VAGINAL ORIFICE, AS DISTINGUISHED FROM LACERATION OF THE PERINEUM.—Anatomically the orifice of the vagina and the obstetrical perineum have no direct connection. They are quite distinct parts, removed from one another by the structures forming the fossa navicularis. It is the orifice of the vagina which I declare to be invariably injured in the natural labour of a primipara. The perineum does not always escape laceration, but it frequently does so. It is not only anatomically that that the orifice of the vagina is distinct from the perineum; it ought to be distinguished practically. When the head is on the perineum, or has passed beyond the outlet of the ligamentous pelvis—"out of the bones," as I have heard midwives call it—it is generally described as retained by a rigid perineum. But this description is plainly often inaccurate, for the perineum has yielded as much as can be expected or wished of it, and yet the head is not born. This condition is seen almost exclusively in primiparæ, and it is the orifice of the vagina, not the perineum, that is undilatable or rigid. (Dr. J. Matthews Duncan, p. 294.)

OVARIOTOMY SUPERSEDED.—When the poles of a battery are placed in an albuminous fluid, clothing and thickening take place at the positive pole, and liquefaction at the negative. The same thing happens if an ovarian tumour is subjected to galvano-puncture. Three cases of cure by this plan of treatment are related by Dr. Semeleder. In the first case, the treatment was continued six months, in the second two months, in the third six weeks. In the first case only did the tumour before treatment was commenced reach above the umbilicus. In the second case the remains of the cyst are described as being hard and of the size of a small apple, p. 315.

PENDULUM MOVEMENT IN WORKING THE MIDWIFERY FORCEPS.—The side to side movement of the midwifery forceps in the act of extracting the child's head, or pendulum movement, as it may be called, is not only useless, but mechanically a mistake. The idea that there is any saving of force, so far as pressure on the mother's and child's part is concerned, by resort to the oscillatory or pendulum movement, is such that I cannot argue against it. The question involved is purely mechanical and of extreme simplicity. It is this: a mechanical difficulty in bringing a child's head through a resisting passage has to be overcome; further, the difficulty is not to be evaded by changing the position of the child's head; on the contrary, that position may be supposed to be the most favourable for facility of propulsion. Now, can any oscillation or other imaginable movement diminish the mutual, and, in this case, injurious pressure or force required to produce advance? The question requires no answer. The supposition is absurd. (Dr. J. Matthews Duncan, p. 290.)

PESSARY.—A new form of pessary, and one worth notice, consists of a watch spring coiled spirally, with the extremities left



free ; this is encased in caoutchouc, and in its external appearance resembles many of the various ring pessaries at present known. The advantages of this pessary will appear by reference to the engraving on the left, where it is shown collapsed for facilitating its introduction ; when *in situ* it expands to the circular form, as shown on the right of the figure, or assumes such other shape as may best accommodate its contact with surrounding parts. The pessary is made in sets of three, and has the additional merit of being inexpensive. It is made by Salt and Son, Birmingham. (Lancet, Dec. 4, p. 807.)

PUERPERAL FEVER.—Of the several ways in which the subtle poison may be conveyed to the lying-in woman, none is more certainly proved than that it may be conveyed by the hands of the practitioner in making vaginal examinations. But this is not the only way. It may probably be communicated through the clothes, instruments, respiration, or even the secretions of the medical practitioner, midwife, or nurse. That it may be so spread, we have ample evidence, if we look back to the records of Gordon, Armstrong, Gooch, Ramsbotham, and others. Dr. Roberton, of Manchester, gives one of the strongest instances possible. In the space of one calendar month, a midwife attended twenty cases belonging to a lying-in charity ; of these, sixteen died of puerperal fever. Other midwives of the same charity attended 380 cases in the same district and at the same time, and none of these were affected by the disease. After the birth of the child, a full and perfect contraction of the uterus should be secured, by seeing that the organ is not emptied too suddenly, and that the hand follows the fundus down from above as its contents are expelled. Subsequently to delivery, the various known methods should be practised to promote the contraction of the walls of the womb and the diminution of the uterine cavity ; and any clots forming, should be removed from time to time during the hour after removal of the placenta. It is superfluous to say that the placenta should be extracted in its entirety, when possible ; and great care should be taken not to leave any detached portions adherent to the uterine walls. The same precautions should be observed, when practicable, in cases of abortion. We are all agreed as to the absolute necessity of preventing, directly or indirectly, any communication between cases of erysipelas and puerperal patients. We are agreed also as to the necessity of the midwifery practitioner avoiding all post-mortem examinations. His hands should avoid all contact with specific poisons or septic materials ; and if perchance his fingers have touched anything suspicious, he should at once

carefully and thoroughly disinfect them. The necropsies which seem most baneful are of those bodies which have recently died of erysipelas, peritonitis, zymotic disease, or any other inflammatory and febrile affections; and in such instances the accoucheur should not even be present in the room when the dissection is made, as, though he decline to touch, yet his person and clothes may become infected by the poison. This care about absolute cleanliness of the hands in attending labours should extend even to occasions when a medical man has had digitally to examine patients suffering from offensive discharges either proceeding from the surface or from the interior of the body. We have heard in this discussion that the discharges from cancer may produce fever in a lying-in woman; and this is probably true of other putrescent discharges. Many chemical substances have been recommended to be added to water for purifying the hands: iodine, chlorine and its compounds, sulphurous acid, cyanide of potassium, carbolic acid, and the permanganates. It matters not which agent is employed, so long as it is used carefully and efficiently; and if it be necessary to disinfect clothing, this is readily done by exposing it in an oven to a high temperature, for which many upholsterers have a suitable arrangement. (Dr. W. O. Priestley, p. 300.)

RAPID DILATATION OF THE URETHRA IN WOMEN.—The great dilatability of the female urethra has been long known, but its rapid dilatation is only of recent date. It can only be undertaken after the administration of anæsthetics. Either the finger or specula, specially designed for this purpose, may be employed after the external orifice has been incised (Simon). The operation is completed in a few seconds; the bleeding that accompanies it is slight and always free from danger. At most it is only sufficient to interfere with the ocular, but never with the digital, examination. The canal may be enlarged by this means almost to the diameter of an inch without danger. Incontinence of urine is a rare subsequent event. There are a number of cases of so-called cystitis and urethritis and of neurosis, with painful contraction of the bladder, resembling vaginismus, in which forcible dilatation proves the best means of cure. In one case Spiegelberg discovered, by means of rapid dilatation, a polypus in the lower part of the bladder, which had been for years the cause of trouble. (Prof. Spiegelberg, p. 225.)

RUPTURED PERINEUM.—If the parts are brought together immediately after delivery the parts exactly fit one another and are so lax that there is no tension on the sutures. The only point for consideration is the selection of the best

material to use for sutures, and the best means of introducing the sutures. Soft silver wire is, as I believe, the very best material, on account of its cleanliness, the very slight irritation it causes in the tissues through which it passes, its inelasticity, and the ease with which it is withdrawn. Soft iron wire is only inferior to silver on account of its tendency to become hard and corroded when left for some time in a wound, and thereby giving some pain in its withdrawal. Silk and twine are less suitable as materials for suture, as they cannot be long retained without exciting suppuration. Catgut, whether carbolised or not, is to be rejected altogether for the operation under consideration. If carbolised, it is liable to undergo solution in the secretions of the wound; while ordinary catgut is uncleanly, and irritating to the tissues. Any form of suture-needle may be employed that will carry the suture through the whole depth of the wound, entering the skin a full inch from one margin, and emerging at the same distance from the opposite. Least painful to the patient is some kind of tubular needle, through which the wire may be passed, such as Startin's or Matthieu's needle. Just as efficient, though causing more pain, is the large sickle-shaped needle, fixed in a handle, having an eye at the point. If such an one be used, it should be thrust through the soft parts without the suture, the latter being threaded when the needle-point emerges from the skin, having completely transfixed both sides of the rent: the needle, as it is withdrawn, will leave the suture in position. For very slight ruptures no such formidable instrument is required as the one above mentioned, since the curved needles that are ordinarily carried in a pocket-case will suffice. In operations performed within a few days of delivery, unless the rent lay open the rectum, the sutures may be fastened by simple twisting; and unless they excite irritation, these may remain *in situ* for a fortnight. When the rectum is laid open, it is more prudent to fasten the sutures to leaden plates, buttons, or pieces of bougie, so as to bring the parts together after the manner of quilled sutures, two or three superficial interrupted sutures being also inserted. (Mr. T. Smith, p. 284.)

SHOULDER PRESENTATIONS.—Dr. Maxson, of Syracuse, New York, accidentally discovered that by raising the hips of a woman in labour so as to be relatively much higher than the shoulders, an abnormal presentation was spontaneously rectified. In a difficult shoulder case he acted upon this idea in the following manner: He says, "I folded several quilts compactly, laying them one upon another to the height of about one foot, and assisted her to kneel upon the quilts.

with her head and shoulders resting upon the bed, and her face forwards, so as to bring her body to an angle with the bed of nearly 90 degrees. I then pressed my hand gently against the shoulder, which readily receded, until I was enabled to clasp the vertex with my fingers, and with the assistance of the next pain to so 'engage' it, that, when the patient was placed upon her left side and the quilts removed, a perfectly natural presentation presented itself. In a few hours the labour terminated in the delivery of a healthy boy, weighing ten pounds. Only a few moments were occupied in the process, and subsequent experience convinces me that *shoulder presentations* can generally be converted in this way into *natural* ones without a resort to 'turning,' and with no risk for the mother or the child." (Dr. P. R. Maxson, Dr. E. Warren, p. 281.)

TUMOURS OF THE MAMMA.—Tumours of the mamma are of two great classes—the malignant and the non-malignant. Simple or non-malignant tumour consists of the collection into one spot of one of the normal constituents of the body. Thus, confining our attention to the mamma, we may have fatty tumour, fibrous tumour, adenoid tumour; and these are simple, because they consist of the same substances which form normal parts of the body. Malignant tumour, on the other hand, is formed of a collection of substances which are a new growth in the human body; of abnormal substances which do not exist in the body of the living man. That is the essence of the distinction between the two kinds of tumour. If you examine the minute structure of the simple non-malignant tumour, you will find it to consist of a modification, it may be of one of the normal tissues of the body; if you examine the malignant tumour, you will find it to consist of tissue of which there is no example in the human body. With regard to the mode of growth, simple tumour extends by increase of bulk, displacing, pushing aside, perhaps causing to be absorbed, the tissues with which it is surrounded. Malignant tumour increases in bulk by incorporating with itself the adjacent tissues, and gradually converting them into itself. Further, simple tumour is for the most part isolated and incapsulated, contained in a membrane which is called its capsule. Real malignant tumour has no absolute defined membrane, but invades all the neighbouring tissue without any well marked margin. The ordinary name of cancer is well given to malignant tumour, because it gradually draws into its centre all the neighbouring parts, so that in the end it extends without limitation. (Dr. G. Buchanan, p. 318.)

MISCELLANEA.

ALCOHOL. — *Its Physiological Action.* — The power of alcohol to lessen oxidation, useless or even injurious in health, increases the value which its other properties give it in the treatment of febrile diseases, where oxidation is going on too quickly, and rapidly destroying the tissues. The very increase of temperature which this oxidation causes helps of itself to accelerate this disintegration of the tissues, for a high temperature causes them to split up, even although they do not undergo oxidation. Thus the albuminous tissues probably become decomposed and yield urea, other nitrogenous substances, and fat. The fat does not undergo complete combustion but accumulates in the tissues from which it has been formed, and thus the heart of patients who have died of pyrexial diseases, instead of being purely muscular, is generally to a great extent fatty. In such a condition of pyrexia alcohol will diminish the excessive waste in two ways. Firstly, it will impede oxidation, and secondly, by thus lessening the temperature, it will diminish tissue-disintegration. From a survey of all the evidence on this subject, I think we may conclude that in moderate doses alcohol undergoes combustion in the body, and will supply energy, yield warmth, and tend to sustain life in the same way that sugar would do, and is therefore to be reckoned as a food. At the same time it has a power of diminishing oxidation which prevents its employment as a food to any great extent in health, but greatly increases its utility in disease. In feverish conditions it diminishes tissue waste, and thus keeps up strength in three ways:—1. It undergoes combustion itself as a food instead of the tissues. 2. It lessens oxidation in them. 3. It lowers the temperature which itself increases tissue degeneration. If alcohol always quickened the pulse in disease as it generally does in health it would probably be injurious in prolonged illness, as it was found to be in prolonged exertions by the soldiers in Ashanti. But this is not the case, for in fever the quick pulse frequently becomes slower after the administration of alcohol, and, indeed, an excellent rule of practice is not to give alcohol if it increases the rapidity of the pulse already too quick. Alcohol thus economises the vital power of the heart, and tends to prevent death from exhaustion. It is difficult to say precisely how the slowing of the pulse is effected. (Dr. T. Lauder Brunton, p. 335.)

ANÆSTHETICS. — *Administration of Ether.* — I have generally found it best to give a little chloroform at first. One drachm

is usually sufficient. I give a drachm as a dose, poured upon a thickly-folded towel, held about two inches from the patient's mouth. After this quantity of chloroform has been inhaled, ether can be borne, in most cases, without giving rise to any choking. There is rarely more effect from this quantity of chloroform than a little mental obscurity. I then give the ether, in doses of an ounce at a time, poured upon a sponge, fixed at the top of a cap-shaped inhaler, made from an ordinary bedroom towel. The inhaler I use has been devised and described by my friend Mr. Lloyd Owen. It must be applied closely around the nose and mouth; I like it to be large enough to include the nose and the chin. But little air must be admitted. The dose of ether must be renewed about every two minutes, removing the inhaler completely from the face, and allowing one or two inspirations of air, until anæsthesia be induced. When the insensibility is perfect, it may be maintained by giving smaller doses of the ether, half an ounce, at less frequent intervals. We can only judge whether to stop or continue the administration by the condition of the patient. We must give enough ether to produce anæsthesia, and we must continue to give it so as to maintain anæsthesia, so long as the operation may last. But it must never be forgotten that even ether is not absolutely safe when given to full anæsthesia. The administrator must never cease to watch the patient closely, observing, with undistracted attention, the breathing, the pulse, and the colour of the face; he must have nothing whatever to do with the operation or the operator, nor even to look at it or him. (Dr. J. Sawyer, p. 344.)

New Method of Administering Ether Vapour.—There can be no question as to the superiority of ether over chloroform—but it has one drawback, viz., the occasional production of pulmonary inflammation, more of the nature of bronchitis than pneumonia. This may be quite prevented by using the vapour of boiling anhydrous ether, pure and free from any admixture of air. The vapour thus given is quite pleasant, and acts in somewhat smaller quantity and more readily than the vapour at the ordinary temperature. An apparatus for this mode of etherisation has been constructed by Messrs. Salt and Son, of Birmingham, and may be obtained from them. (Mr. Lawson Tait, p. 341.)

CHLORAL AS AN ANTISEPTIC.—Hydrate of chloral applied in solution externally has powerful antiseptic properties. It has been found as the result of experiment that a lotion containing from 5 to 15 grains of the hydrate of chloral to the ounce of water, formed an excellent dressing to ulcers and wounds, dressed with lint and gutta-percha in the ordinary

manner. I use chloral solution as an injection into the sacs of large abscesses, and found that it tended much to diminish secretion and make the parts heal. I found it also a useful lotion for the eye in inflammatory conditions of that organ. It is an excellent application to burns, and very specially where there is a foetid discharge. I also found it a good application to remove warts from the hands and fingers. I used for this purpose a lotion containing 15 to 20 grains to the ounce of water, applied by means of lint and gutta-percha. It causes no pain, and the wart speedily becomes smaller, and gradually disappears. I also used it as a lotion to sore nipples and to inflamed mucous membranes. (Dr. W. Craig, p. 363.)

CHLORIDE OF LEAD AS A DEODORISER AND DISINFECTANT.—

Chloride of lead is the most powerful and economical agent for eliminating sulphide of hydrogen from the atmosphere as well as from all organic matter in a state of decomposition or putridity. To prepare it for use, take, for ordinary purposes, half a drachm of nitrate of lead, dissolve it in a pint or more of boiling water—(nitrate of lead is a soluble salt, and very cheap; it may be had in any quantity for about a shilling a pound, and should be much cheaper if bought in large quantities),—and dissolve two drachms of common salt (chloride of sodium) in a pail or bucket of water; pour the two solutions together, and allow the sediment to subside. The clear supernatant fluid will be a saturated solution of chloride of lead. A cloth dipped in this solution and hung up in a room will sweeten a fetid atmosphere instantaneously, or the solution thrown down a sink, water-closet, or drain, or over a heap of dung or other refuse, will produce a like result. Even the tarnishing of gold and silver plate may be prevented by a rag dipped in the solution being hung up in the room or window where it is exposed. It will thus be seen that one great advantage of chloride of lead over all other deodorisers lies in its great cheapness. The cost of the quantity of nitrate of lead required for use may be estimated at a fraction of a farthing; it is easily carried about, or may be sent by post; it is a dry solid, is not caustic, nor will it discolour or blister the skin; it can be kept in paper or a pill box, and therefore does not require a glass or other fragile bottle. (Dr. R. H. Goolden, p. 51.)

SALICYLIC ACID.—Salicylic acid is three times as powerful in its anti-fermentative action as carbolic acid. Experience has proved that it is equally reliable as a preventive and corrective of putrefaction when applied to sores and wounds, and that it materially assists reparative action. Salicylic acid is a trustworthy antiseptic. But carbolic acid has been proved

beyond doubt to be so also, and boracic acid is largely used for similar purposes; therefore, unless salicylic acid possesses advantages over both, its substitution for either cannot be justified. It does, however, seem to me to possess certain advantages, which, though few in number, are not of less importance on that account; they may, I think, be best elicited by comparing carbolic and boracic acids with it. The two manifest objections to the use of carbolic acid, valuable agent though it be, are its irritating action and its smell. Salicylic acid can hardly be termed an irritant, and preparations containing only a small percentage of it can be thoroughly relied on as trustworthy antiputrescents, and it is odourless. As an application to granulating surfaces a lotion containing 1 part of acid, 3 of sodium phosphate, and 50 of water has been found useful. As already mentioned, heat and the addition of borax increase its solubility; a clear lotion of considerable strength can thus be obtained: for instance, 10 grains of acid can be readily dissolved in one ounce of water by heating and then adding six grains of borax, or even 20 grains of the acid will be taken up by an ounce of water if 14 grains of borax are added. These solutions, in addition to their use as lotions, may be used as gargles, where an antiseptic and astringent gargle is indicated. To a surface where it can be accurately applied, and where the discharge is slight, though I have tried it with excellent effect where it was profuse, the cerate suggested by Prof. Lister for rodent ulcer—salicylic taking the place of boracic acid—is a nice application. The formula is from half a drachm to a drachm of salicylic acid, one drachm of white wax, two drachms of paraffin, and two drachms of almond oil; melt and rub up in a heated mortar. It should be spread on strips of muslin or fine linen. Another ointment may be made of sperm oil, one drachm and a half; oil of theobroma, five drachms and a half; salicylic acid, from half a drachm to a drachm. This forms a thick paste, which should be thickly spread on lint. A very simple and most useful ointment, and one which answers admirably in some affections of the skin, is formed of half a drachm to a drachm of the acid to seven drachms of simple ointment. Though neither watery nor oily preparations of this acid belong to what is termed elegant pharmacy, yet a liniment of salicylic acid and olive oil will be found of much efficacy in burns. I cannot refrain from urging its use in eczematous affections, for nowhere is its beneficial action more marked than in them. In the eczema of children, more especially in that found affecting the head and face, it is far superior to any remedy I have yet employed. (Dr. J. C. Ogilvie Will, p. 40.)

PRACTICAL MEDICINE.

DISEASES AFFECTING THE SYSTEM GENERALLY.

ART. 1.—SOME CONSIDERATIONS RESPECTING INFLAMMATION.

By Dr. C. HANDFIELD JONES, F.R.S., Physician to St. Mary's Hospital.

If we endeavour to form some idea of the vital changes taking place in a part which is becoming inflamed, we may safely assume as a basis for our consideration the following facts: (1) That every arteriole has a contractile muscular coat, which, in the healthy state, is moderately contracted, and is kept so by nerve influence. The existence of a power of active dilatation, though affirmed by some good observers, does not appear to me at all established, and to be rendered unnecessary by the dilating pressure of the contained blood. (2) That the capillaries which surround the tissue elements possess a certain normal retentive power, varying in different parts, which prevents more than a very small amount of blood fluid from filtering, as it were, through their homogeneous walls. This retentivity is weakened by excess of intravascular pressure, whether from venous obstruction or arterial relaxation, and is also probably more or less impaired by loss of or by morbid nervous influence. The common expression, "tone of the vessels," comprehends both arterial contractility and capillary retentivity. The capillaries are filled with moving blood, which, as the carrier of poisonous matter, may no doubt act injuriously on and disorder the tissue, and, on the other hand, may be morbidly affected by the tissues themselves. To determine more precisely the rôle of the blood, apart from the tissue, is hardly possible at present. (3) That the tissue elements have a special vital power of their own, by which each part is qualified for its special function. These powers may be distinguished for brevity's sake as arterial, capillary, and nutritional, or tissue power. A fourth power also is present in most, if not in all, parts—viz., the nervous.

This may be regarded as influencing more or less decidedly the arteries, the capillaries, and the tissue. The arteries are always influenced by it; the tissue often very positively, as in the case of muscles; the capillaries less manifestly, except in the case of the lacrymal, salivary, and urinary glands. It may be true, as Ludwig and Hafiz maintain, that the arterial nerves of a muscle have a distinct origin and course from the proper motor nerves; but there can be little doubt that their central extremities are so connected in the spinal cord or elsewhere that they can influence each other. Now, in the state of health, as we know, a relation varying within certain limits subsists between the vasal powers on the one hand, and the tissue power on the other. This relation is one of *inverse* variation of their manifestations, increased activity of the tissue coinciding with lessened of the vasal elements, and *vice versa*. This alternation can hardly be referred to any other mechanism as its efficient cause than the nerve or nerves with the centre. It is highly probable that nerve force can be converted into tissue force, and it would seem that the active state of the tissue in some way absorbed or monopolised the larger amount of the nerve force generated at the centre, so that the other filaments supplying the vessels were left in a minus state, and were unable to prevent the arterioles from dilating under the pressure of the blood column. The more active the tissue the greater will be the degree of vascular paresis, which, in such cases as severe chorea and tetanus, is known to be sufficient to allow of actual extravasations of blood occurring in the affected nerve centres. When the tissue ceases to act the vasal nerves regain force, the vessels again contract, and the blood supply is diminished. So far all is normal. But if the tissue excitement be excessive and abnormal, if the functional power be lost in morbid and purposeless action, the nerve will remain in a state of paresis, the vessel relaxed, and the hyperæmia will be persistent. Fluid and corpuscles will escape from the capillaries, and inflammatory products make their appearance. Admitting the correctness of the observations, which go to prove that the corpuscles of an inflamed part proliferate and generate pus or exudation cells, we have herein a proof of the disturbed, disordered vitality of the affected part, which we have made the prime motor of the inflammatory process. Such inflammation as this I termed long ago tissue inflammation, to mark that in it the disorder of the tissue was primary. All traumatic inflammations are of this kind, all mechanical and chemical, and those produced by irritants locally applied. In proof that the tissues are specially affected in these may be cited the fact that the hyperæmia is precisely coterminous with the extent of the irritation. Thus a square

blister will make a square patch of inflammation; a linear will make a linear; an annular an annular; and so on. If the hyperæmia depended on primary neuro-vasal paralysis this could hardly be; the hyperæmia would be diffuse, and would affect areas corresponding to the distribution of nerves and arterioles. In many instances, however, it is probable that the tissue disorder affects the capillaries solely or principally, and the arteries and their nerves but little. A cell lying close to a capillary may well affect it directly, and not through the intermedium of the nervous system. Certain limited inflammations, as those of psoriasis guttata, are probably caused in this way. The following instance affords evidence to the same effect: A patient had been painted with linim. iodi over the left subclavicular region, and the skin had become in consequence pretty smartly inflamed. To allay the irritation a poultice was ordered, but being maladroitly applied it only covered about two-thirds of the inflamed area. In two or three days the part which had been poulticed was nearly well, while the non-poulticed third was still much inflamed. Here the effect of a remedy in reducing inflammation was confined to the portion of tissue on which it acted, and left the adjacent uninfluenced. Another argument for the view that the tissue in many instances of inflammation takes the initiative may be drawn from the local action of certain drugs, which inflame particular parts and leave all others intact. It can hardly be anything except the special tissue of the part which determines this result, for nerves and vessels and connective tissue are the same everywhere, and presumably will be affected in the same way by the same agent. When arsenic, used for some remote skin disease, inflames the eyelids, and when the same agent administered for a conjunctivitis effects a cure, it is very difficult to explain this result, except by admitting that in the first case it has acted as an irritant to the tissue, and in the second, as a tonic to the nerves and vessels. Again, when an agent which has no tonic effect on nerves and vessels positively cures an existing inflammation, we are almost obliged to admit that it must have done this by exerting an influence over the tissue of the part which was primarily disordered. Thus, when potass. chloras cures a stomatitis, or veratrum viride a laryngitis, or antimony an inflammatory skin disease, we cannot believe that this has been accomplished by these drugs exerting a tonic action on nerves and vessels, for we never find them to possess such—rather the reverse. But it is quite conceivable that they may annul the morbid action going on in the inflamed tissue, may act as tissue sedatives, like moist warmth, or a poultice.

This, then, is one form of inflammation, but not the most

frequent. More commonly we meet with conditions where the neuro-vasal paralysis seems to be primary, and to depend on the direct action of a poison on the nerves and vessels. The tissue may also be affected, and often is; but its derangement is more of a passive than active kind, and does not induce the hyperæmia. The capillary retentivity is much impaired, and fluids and corpuscles escape abundantly. When the effusion is abundant hyperæmia may not be apparent, and the disorder assumes the aspect of flux. Such inflammations may be termed *neural* to mark their relation to vaso-motor nerve paresis. They are often attended with very marked indications of general prostration.

The degree in which the tissue suffers in different inflammations varies very remarkably. We may take, on the one hand, as a typical instance, the inflammation of common decubitus. Here we first observe the skin to be red and tender in a circumscribed area, which, however, can be made pale by pressure. Soon the blood stagnates, and is perhaps extravasated in a central patch, the epidermis becomes detached, and the tissue dies. A sore results in which the same process is apt to continue. That the pressure is not the chief cause is evident, because such effects only ensue in conditions where the general vital power is gravely depressed, as by a low fever, diphtheria, phthisis, &c. Hyperæmia alone is, of course, insufficient. The stagnation of the blood, which some will designate capillary embolism, might appear an adequate cause did we not remember that quite the same changes often occur in purpura without any sloughing or ulceration, and, moreover, that stagnation is not a constant occurrence. No complete explanation of the local decay and death can be rendered: all that can be said is that the pressure acting as an irritant, or injuriously, on weakened vessels and tissue, has paralysed the one and killed the other. Contrast with this another typical instance—viz., the hepatised lung in acute sthenic pneumonia. Here the delicate membrane of the air-cells, $\frac{1}{20000}$ inch in thickness, with its woof of capillary vessels, is imprisoned amid a massive exudation, which, by its mere pressure might well have been expected to destroy the delicate fabric, and especially to have involved it in its own decay. Yet, as a writer on inflammation says, “goes like snow in sunshine,” and leaves the normal tissue none the worse. It is not, of course, denied that even in this instance the qualities of the tissue do not suffer some temporary impairment, as attested by the softening which in more asthenic states is often so marked. But, admitting this, there remains nevertheless a very striking contrast between the behaviour of the tissue in the two cases cited, and this shows what a very

important part the vitality of the tissue plays in determining the results of inflammation.

Another typical instance for consideration is that of the common abscess, boil, or whitlow. Here also the tissue affection seems to be the leading phenomenon. When once the morbid action has fairly started it progresses almost invariably to suppuration and discharge of matter before recovery takes place. It seems as if the effective cause so deranged the nutrient process in the affected part, so perverted the normal tissue force, that until the abnormal energy thus evoked was spent in the organisation of a multitude of cells—pus corpuscles—the vasal nerves could not resume their sway over the arterioles and capillaries, and put an end to the hyperæmia and the effusion. In many instances of this kind the hypothesis of a poison to be eliminated by the pus may be entertained as the effective cause; but in many also, especially those where the abscess is of traumatic origin, it seems that the derangement of the tissue is the direct result of the injury. The same cause may—acting in a similar way on the tissue—start that abnormal mode of growth which we call cancer. A tendency to the occurrence of such suppurations prevails at times somewhat epidemically, and is regarded correctly as an evidence of depressed vitality. Certainly one may say that, *cæteris paribus*, the sounder is the constitution, and the stronger the vital force, the less tendency is there to suppuration from wounds or other provocation, and the converse is also true. The pyogenic diathesis is not, I believe, a state characterised by the presence of a pyoid material in the blood, but simply one in which the normal tissue force is unstable, and is easily deranged.

There is a great difference between these acute pyogenic inflammations and similar but chronic processes on mucous surfaces, which are often most evidently controlled and put an end to by tonic remedies. The cause of this difference lies probably in the tissue being primarily affected in the first, and the vessels and nerves in the second.

The next variety of inflammation for notice may be the rheumatic, which is characterised by its shifting tendency and disinclination to form pus. By the first of these characters it shows an affinity to nerve disorders, as it does also by the great amount of pain with which it is attended—often quite out of proportion to the degree of inflammation existing. Its reluctance to suppurate testifies to the same effect, for this is a feature also of some inflammations which arise out of neuralgia. The replacement of articular rheumatism by active delirium, not depending on meningitis, is another evidence of the same kind, and so, I think, is the association of chorea with or its development out of rheumatism. The non-febrile forms of

rheumatism pass quite gradually into typical neuralgias, and cases often occur where it is hard to say whether there is a rheumatic element in the neuralgia or not. Erythema nodosum is certainly a rheumatic inflammation, and if I attempted to define wherein this cutaneous disease differs from a common suppurating furuncle of the skin, I should say that in the first the vessels and nerves were chiefly at fault; in the latter, the tissue. It seems probable that a peculiar kind of neuro-vasal disorder has more to do with shaping the phenomena of rheumatism than the actual cause, or motor. For, as there is a scarlatinal, a gonorrhœal, a summer rheumatism (caused by heat), and a winter one (caused by cold and wet), it is evident that the cause may vary very much without the resulting phenomena being materially different. So it is with jaundice.

Gouty inflammation has much resemblance to rheumatic. Like the latter, it is fugitive, extremely painful, non-suppurating, and shows a decided tendency to alternate with nerve disorder. Gouty migraine, asthma, neuralgia, and mania are well known, and not rare occurrences. The dependence of gouty inflammation on a special toxæmia is a matter of tolerable certainty. According to the view I have long held that pain is a form of sensory paralysis, the first step in gouty inflammation appears to be the setting up of irritation of sensory nerves. This causes inhibitory or reflex paralysis of vaso-motor nerves, and consequent hyperæmia of the affected parts, attended with œdema, and followed by desquamation. The circumstance dwelt upon by Dr. Garrod, that even intense gouty inflammation, the part being as red as scarlet, "never leads to the formation of pus," is very instructive. It shows that neuro-vasal disorder is insufficient for the production of the graver results of inflammation; serous effusion indeed takes place, and necrosis of the outer layers of the epidermis; but the true tissue of the part is not involved in the morbid action until a local cause is added in the presence of chalky deposits from previous attacks.

In catarrhal inflammations, whether of cutaneous or mucous surfaces, the chief features are diffuse hyperæmia, with more or less copious escape of fluid and corpuscles from the blood, and detachment or malformation of the surface layer of the tissue. The exudation is sometimes wanting, as in dry eczema and dry catarrh, but, nevertheless, the nutrition of the cell-growth covering the surface, the epidermis, or epithelium, is deranged. The great majority of these disorders are characterised by general debility and vaso-motor nerve paresis; but there are certainly instances not altogether rare where this is not the case. Weak or languid heart, weak arteries, weak capillaries, weak tissues, might serve as a general formula; but

in some instances one or other of the factors would have to be modified. Thus, we may have vigorous heart, or retentive capillaries, or stable tissues, in instances where all the other factors are weak. In eczema the tendency to implication of the tissue seems to be greatest in the "rimose" form of the disease, where hyperæmia is mostly by no means considerable. In some instances nothing can be more marked than the tendency to arterial paresis, and flushing of the capillary plexus with blood, often associated with leakage from the latter. The capillary power seems to be specially defective, more than the arterial, in the common impetigo of children, where much corpuscular exudation occurs in limited patches, but with little diffuse hyperæmia. In many bronchial catarrhs with thick muco-purulent secretion the same is probably the case. In all this class of instances the tissue seems to be but little implicated compared to the nerves and vessels, it rarely ulcerates or undergoes any permanent change. Detachment and casting off of its superficial cell-layer is all that mostly ensue. When the nerves and vessels regain their normal status, the tissue quickly returns *ad integrum*, and, for the most part, is not pigment-stained, as often occurs in psoriasis. This latter malady affords an interesting contrast with the preceding. The absence of exudation, the abrupt limitation of the hyperæmia and swelling, the gradual extension of the patches peripherally with their recovery in the centre, the frequently good state of health, are all points in which more or less the different character of the two is displayed. In the majority of cases eczema and its congeners are produced by some influence acting *ab externo*, probably by the unknown cause of catarrh, sometimes, however, by heat. Psoriasis, on the other hand, is produced by some in-bred poison, sometimes the gouty, which seems to irritate and derange the nutrition of the tissue rather than to cause direct paresis of vasal nerves. The annular or arcuate arrangement which the eruption of psoriasis often exhibits affords considerable evidence that the tissue is chiefly affected. It is almost impossible to conceive a paralysis of vasal nerves producing hyperæmia of the limited extent and peculiar form that is often seen in psoriasis. The absence of exudation also indicates that the tissue is affected rather than the vessels. For, if these were at fault, exudation more or less would almost necessarily occur; whereas the fact seems to be that a morbid attraction is exerted by the tissue on the contents of the capillaries, leaving the vessels themselves intact.

In erythematous inflammation the chief peculiarity is the slightness of any change beyond that of the distribution of blood. Accelerated necrosis and detachment of the epidermis occurs to a greater or less extent, but no apparent exudation

or other morbid alteration. Between such inflammation as this and the destructive inflammation of decubitus or phagedæna the distance is immense. What makes the difference is not the degree of hyperæmia, but the state of vitality of the tissue.

The object of the foregoing remarks is to lay stress (1) on the primary concernment of the tissue in many instances of inflammation; (2) on the same being true of the vasal nerves in many others; (3) on the varying quality and results of inflammation, according as the affection of the tissue or nerves predominates.—*Medical Press and Circular*, Aug. 4, 1876, p. 83.

2.—INOCULABILITY OF SPECIFIC FEVERS.

By the EDITOR of the BRITISH MEDICAL JOURNAL.

Odessa having been subject, during the last three years, to frequent epidemics of typhus, typhoid, and relapsing fevers, Dr. Motschutkoffsky, physician to the City Hospital, has availed himself of the opportunity to study the question of the inoculability of these different forms of fever; being encouraged to undertake the investigation by a successful inoculation which Dr. Münch, the prosector to the hospital, made on himself with the blood of a patient suffering from relapsing fever. The experiments were made on persons who submitted to them voluntarily, and on animals (monkeys, rabbits, dogs, and cats). Inoculations from typhoid and typhus patients were frequently made, but never succeeded either in the human subject or in animals. On the other hand, relapsing fever is readily conveyed by inoculation to a healthy person. The inoculation on animals were without result.

As inoculable material, the blood alone was found suitable—repeated inoculations with milk, sweat, urine, saliva, and excrement, being resultless. The inoculation only succeeded when the blood was taken from a patient in the period of pyrexia, irrespective of which period it was. Inoculations with blood from patients who were free from fever during the stage of intermission gave negative results. When the blood was taken during a feverish attack, the inoculations succeeded, whether spirilla could be seen under the microscope or not: they succeeded, for example, even in the first hours of an attack which was beginning. The relapsing fever, thus artificially produced by inoculation, differed in no respect from the relapsing fever acquired in the ordinary way—neither in the clinical appearances, nor in the strength, duration, or number of the attacks.

The blood of the inoculated disease was again inoculable; but the theory of Davaine, regarding the increased potency of

suppurative material when re-transmitted by inoculation, was not borne out. From inoculation with the blood of relapsing fever patients, relapsing fever was exclusively produced, and never any other form of infectious disease. Blood from patients suffering from the bilious type of relapsing fever produced the fever, but without the bilious type.

The period of incubation never lasted less than five nor more than eight days. The duration of the period of apyrexia was approximatively equal to the period of incubation. Inoculation with blood taken during the incubation period from a person inoculated was without result. The quantity of blood used in inoculating had no influence either on the duration of the incubation or on the intensity of the attacks. Blood taken from a person ten weeks after recovery from the last (fourth) attack did not produce relapsing fever. Blood kept for two days in a closed capillary tube at the temperature of 54 deg. Fahr. gave positive results: in this case the spirilla had not lost their capacity of movement. Inoculation with blood diluted with an equal part of 0.1 solution of quinine in water, acidulated with hydrochloric acid, succeeded completely: the spirilla lost their capacity of movement immediately after the mixture of the blood with the solution. Ten parts of blood diluted with one part alcohol at 60 deg. gave negative results after inoculation, the spirilla in this case also immediately becoming motionless when the fluids were mixed.

The results of Dr. Mutshutkoffsky's investigations are given in a short paper in the *Centralblatt* of March 11th, which we have translated almost literally. It is satisfactory to learn from the remarks with which the paper concludes, that none of the persons who were the subjects of experiment suffered from any permanent derangement of their health in consequence of the fever. The greater number of the experiments were made in the presence of the other members of the hospital staff. The author promises a further and full account of this remarkable investigation.—*British Medical Journal*, March 25, 1876, p. 383.

3.—DR. KLEIN'S REPORT ON THE CONTAGIUM OF ENTERIC FEVER.

In a report issued last year by the medical officer of the Privy Council, Dr. Klein pointed out that he had discovered the presence of peculiar bodies of the nature of minute vegetable organisms in the early stages of enteric fever. Since then, a principal object which Dr. Klein has held in view in his further studies of this subject has been to ascertain whether there is any special relation between these organisms and the

progressive changes which take place in the intestine and diseased parts in this fever, similar to that which has already been shown to exist in the case of small-pox of sheep. This has involved much study of the microscopical anatomy of the mucous membrane and of the lymphatic follicles of the ileum, and incidentally a considerable amount of valuable information has thus been obtained as to the minute structure of the parts examined. In the sixth volume of the new series of Mr. Simon's reports, Dr. Klein's results are published in detail. They include an interesting account of former observations with regard to the minute anatomy of the organs most implicated in enteric fever, and of the more recent discoveries which Dr. Klein has himself made in this subject. Not only these, but also the more remarkable results to be referred to, were, however, not obtained without considerable difficulty, because such microscopical examinations as it was essential to make in order to arrive at them, are all but impossible, unless the parts examined be dealt with almost immediately after death; and in most hospitals a regulation to the effect that *post mortem* examinations can only be made after a lapse of at least twelve hours from the time of death, has seriously interfered with these researches.

With regard to the principal object under investigation, it should be noted that the facts which have been put together by the most able and experienced pathologists tend to show that the contagion of enteric fever is due to a specific and living organism which, when transmitted from a diseased to a healthy individual, produces the same disease in the latter; and, further, that the chief, if not the only, vehicles of poison are the ejecta of the bowels of an infected person. Contagion in the form of a living organism could not be expected to produce infection at once; and the theory that it is organised is *primâ facie* justified by the circumstance that a period elapses between the reception of the contagium and the manifestation of the disease, during which the poison apparently lies dormant, but is in reality ripening towards an active condition. According to Dr. Klein, no other view of the poison affords any explanation of the incubation-period. But hitherto no one has succeeded in pointing out any specific organic form as the probable cause of enteric fever, and hence the paper which Dr. Klein has prepared is one of the highest interest.

Preliminary investigations were made with the stools of enteric fever patients, and the microscopical appearances which they present are described at length in Dr. Klein's paper. Amongst the objects noticed are numerous bright, highly refractive, spherical micrococci of varying size, both isolated and in chains or necklaces, and at times rod-like structures

from which these micrococci could be traced to originate; but it is not in the evacuations alone that these bodies are found; they have been observed in abundance in the mucous membrane of the ileum in the stages of the disease preceding general ulceration. In those parts of the ileum which, at the commencement of enteric fever, appear to the unaided eye only to be slightly increased in thickness, Lieberkühn's crypts are seen to contain in their lumen, in smaller or larger masses, corpuscles of greenish yellow colour, highly refractive, varying in form and also in size from about twice the size of a human blood-corpuscle to that of a minute granule, and it is evident from the appearances which they at times present that they multiply by transverse division. From the characters which they present, Dr. Klein arrives at the conclusion that they must be of the nature of organisms, and with regard to them he further states that we have to do "with a fungus which possesses mycelium-threads of very unequal joints." In some parts of these threads, probably the terminal parts, "their contents split into macrogonidia or microgonidia," and the gonidia, when discharged, undergo rapid division, so as to form a kind of zoogloea. In short, a very definite conclusion is arrived at with regard to the identity of the contagium of enteric fever with a low vegetable organism.

The products of the fungus are also found in other parts of the mucous membrane. The gonidia form and the micrococci may be seen in the tissue of the mucous membrane close to Peyer's glands, and the micrococci especially occur in large masses in the lymph-spaces surrounding Lieberkühn's crypts and the tissue next to it. The spores and micrococci also find their way from the surface through the Lieberkühn's crypts into the lymphatics and blood-vessels. Both the organisms themselves and the various shapes and positions they assume are depicted in the clearest manner in the diagrams which accompany the report. Indeed, these beautiful illustrations from Dr. Klein's own pencil bring out the results of these investigations in a manner which must be most convincing to the eye of the anatomist and microscopist. One diagram shows how masses of these micrococci penetrate through the epithelium and accumulate in a space between it and the stroma of a villus; in another, the organisms are seen to penetrate from the free surface into the mouth of a Lieberkühn's crypt; they are also seen to make their way into lymph-places and into the walls of veins.

From these circumstances, Dr. Klein considers it clear that we have to do "with an absorption of masses of micrococci from the surface into the lymphatics and blood-vessels;" and he further shows that it is impossible that their presence can be

due to *post mortem* changes. These organisms, too, are identical with those observed in the alvine discharges of enteric fever patients, and, as we observed in a preliminary notice of this report, the appearances presented by the organism "correspond closely with those described by Cohn as characteristic of the vegetation discovered by him in well-water in a district of Breslau famous for enteric fever."

Such results as these naturally give to this paper a special and distinctive interest; and, although, owing to the failure to communicate enteric fever to any of the lower animals, it has not yet been possible to perform, in relation to the supposed contagium of this disease, such crucial experiments as have been made with regard to some other diseases, yet a perusal of the facts recorded by Dr. Klein can leave but little doubt as to their reality, and, in the opinion of Mr. Simon, whose judgment in such a matter carries great weight, the interpretation which has been assigned to them seems to follow as an inevitable consequence.—*British Med. Journal*, March 25, 1876, p. 383.

4.—TYPHOID FEVER, AND THE FATAL EFFECTS OF DIGITALIS.

By Dr. WILLIAM MURRELL.

The treatment of the acute specific diseases by large doses of digitalis has for some years found favour on the continent, although it has as yet been but little used in this country. The drug, according to Wunderlich, is especially efficacious in the second or beginning of the third week of typhoid fever, when the temperature is high and the pulse rapid. It is not claimed for the drug that it has any specific action upon the intestinal lesion, and it is said to be of most value when danger is apprehended from the intensity of the fever. It is usually administered in the form of an infusion, or in substance, but hypodermic injections of digitaline are occasionally given. The quantity required to produce a decided effect upon the temperature and pulse is for an adult from 2 to 4 grammes ($3\frac{1}{2}$ to 3 j.) of the powdered leaves, this dose being spread over four or five days. Larger doses are, however, sometimes employed; Hirtz gives from 30 to 60, and Hankel from 47 to 62, and occasionally as much as 100 grains per diem. If the medicine be given irregularly, or if long intervals intervene between successive doses, the quantity is increased. Wunderlich finds that the temperature is slightly reduced on the first day, but that the pulse remains unaffected till the second or third day. The fall in temperature on the first two or three days is usually slight, but on the fourth or fifth day, when the full influence of the drug becomes manifest, there is often a

reduction in the evening temperature of from 2 to 3 degrees. The temperature is seldom reduced to the normal, and never falls below it. On discontinuing the medicine the fall is at once arrested. When the temperature again rises after any considerable reduction has been made, it never reaches its former height. This second rise is often the forerunner of a definite improvement in the patient's general condition. The influence of the drug upon the pulse is most marked on the fourth or fifth day from the commencement of the treatment, there being often a fall of from 30 to 60 beats in the twenty-four hours. The pulse often falls below the normal, and this effect occasionally persists for some weeks after the termination of the illness. The change in the pulse-rate is coincident with the fall in the temperature. Wunderlich's conclusions have recently been confirmed by Bernheim, who, however, differs from him on a few comparatively unimportant points. Thus he found, that although the pulse and temperature do not fall uniformly, yet the pulse rarely commences falling before the temperature, or the temperature before the pulse. He also states that it is the rule for the pulse to fall below the normal. These views have not, however, received universal acceptance even on the continent. Thus M. Desnos, of the Hôpital de la Pitié in Paris, found digitalis of little or no use in reducing the temperature in enteric fever or for otherwise modifying the course of the disease. He attributes its supposed success in other hands to a therapeutical illusion, and thinks that sufficient attention has not been paid to the natural course of the disease.

In January, 1874, the digitalis treatment was adopted at University College Hospital in a case of typhoid fever; the patient was in the children's ward, under the care of Dr. Ringer, by whose permission these notes are published. The progress of the case serves to illustrate the influence of the drug upon the pulse, and its fatal termination demonstrates the existence of a danger which has apparently been completely ignored by continental observers. The patient (F. F.) a bright, intelligent-looking girl aged ten, had been an inmate of an Orphan Asylum in the suburbs. From the statement of the matron, it appeared that some six or eight weeks previously a child suffering from typhoid fever had been admitted to the Home, and for a few days, before the nature of the illness was recognized, had been allowed to associate with the other children. In about a month, some sixteen or eighteen of the inmates were attacked with the fever, the majority being subsequently distributed among the metropolitan hospitals, a few only being treated privately. It was impossible under the circumstances to obtain any definite information respecting

the onset of the illness; but the patient was stated to have been ill six days.

When first seen (January 2nd, at 7.0 p.m.) patient was lying on her back in bed, her countenance wearing a marked expression of illness. The face was not flushed, but there was a dark rim beneath the eyes; she was quite sensible, and answered quietly and rationally. On being questioned, she said she had a headache, but did not complain of it spontaneously; she had also a pain in the right iliac region, but there was not much tenderness, and no gurgling was felt; there was no rash observable on the thorax, back, or limbs; the spleen was slightly enlarged; there was no lung or heart complication; temperature (rectum) 104° , pulse 130, quick (*i.e.*, of short duration), easily compressible; skin moist; tongue moist and covered with white fur, except at the tip and edges; urine high coloured; sp. gr. 1029; free from albumen; bowels confined; no coryza; no sore-throat.

Although not a typical case of enteric fever, no doubt was entertained respecting the diagnosis. On January 4th (eighth day), at 7.0 a.m., patient commenced taking pulv. digitalis gr. j. every two hours. That night there was no evening exacerbation of temperature, which was a degree lower than at the same hour on the previous night. The evening pulse was 116, as compared with 132 of the night before. The bowels were opened once, the motion being formed and of a dark brown colour. On the following evening a further fall of 16 beats in the pulse was noted, and a reduction of 6-10ths of a degree in the temperature. The patient usually took about 8 grains of the digitalis in the twenty-four hours, the rule being not to give it at night if she were asleep. On the 6th there was a further fall of a degree in the 11.0 p.m. temperature. The pulse continued strong and regular. The bowels were again opened. On the 7th, patient heaved very much after taking the powders, but managed to retain them. There was no further reduction in the pulse, but the 11.0 p.m. temperature showed a fall of another degree. On the 8th, patient vomited twice immediately after taking the medicine, and an equivalent dose (9 M) of the tincture of digitalis was accordingly substituted. There was during the day no further fall in the temperature or pulse. Patient had two motions, not formed, of a yellowish-brown colour, highly offensive odour, and alkaline reaction. From this time the bowels were opened daily, the motions being usually of the character just described. On the morning of the 9th (and the thirteenth day of illness), two spots—rose-coloured approaching brown, about the size of a pin's head, slightly elevated above the surface, disappearing on pressure—were seen in the right lumbar region, and one just

above the level of the first true rib. At 11.0 a.m. the pulse was 66, sharp, not compressible, and for the first time irregular. The irregularity consisted of the intervention of three or four semipulsations between every third or fourth beat. The pulse at 7.0 p.m. was 70, and at 11.0 p.m. 64, showing a fall of 68 beats in six days of the digitalis treatment. The respiration had also been reduced from 32 to 20. At 11.0 p.m. the pulse was 64, there having been a fall of 44 beats in twenty-four hours. On the 10th the pulse varied from 56 to 60, was full and sharp, and the irregularity had disappeared; respiration 16; there was a further eruption of typhoid spots, and an increased elevation of temperature; a trace of albumen was observed in the urine. On the 11th, patient was ordered brandy, 2 ozs., the digitalis to be continued as before. On the 12th, at 11.0 p.m., the pulse was 52 and respiration 32. There was a copious eruption of sudamina on the abdomen and front of the thorax. In the afternoon the temperature rose to 103.8; the patient was rolled over on her side, and her back was examined; there were no bed-sores, and only a few typhoid spots; there was no obliteration of the breath-sounds, no râles, and no dulness at bases; there was no cardiac mischief; the pulse varied from 52 to 60, being at night quick, irregular, and compressible. On the 14th, patient was very restless, and complained greatly of thirst; there was a farther elevation of temperature, which at 11.0 p.m. was 104.4, the pulse being 72, slow, slightly irregular and not compressible. At 3.0 p.m., in consequence of the rise of temperature, an addition of 3 \mathfrak{M} was ordered to the tincture of digitalis, but by mistake the medicine contained only 3 \mathfrak{M} , and of this patient took three doses, at five, seven, and nine o'clock. The error was then rectified, and at 11 p.m. patient commenced taking the full 12 \mathfrak{M} dose. On the 15th there was a slightly increased elevation of temperature, and the pulse, taken five times during the day, was 70, 106, 80, 68, and 90; it was fairly strong, irregular, but not intermittent. Patient was extremely restless during the night, and passed urine and fæces in bed. She was seen at 11.0 a.m., when the temperature was 104, the pulse fairly strong, but intermittent; half an hour later attention was drawn to the patient by a slight gurgling noise; the pupils were widely dilated; the pulse was extremely weak and could be felt with difficulty at the radials; in three or four minutes the patient was dead. There were no convulsions; the temperature in the rectum immediately after death was 104. An autopsy was made thirty-six hours after death. In the lower third of the small intestine, Peyer's glands were inflamed, and marked at intervals with small ulcers, varying in size from a pin's head to a split pea. In places the ulceration had exposed the transverse

muscular fibres of the intestine. There were no signs of perforation; lungs and heart were normal.

Practically, what we learn from the case is this: that although by means of digitalis we may rescue a patient from the deleterious influence of an excessive rise of temperature, yet that its use is not unattended with danger, and that during its exhibition death may occur suddenly and unexpectedly, and without a single note of warning.—*Practitioner*, Nov. 1875, p. 345.

5.—THE USE OF SACCHARATED LIME IN TYPHUS FEVER AND OTHER COMPLAINTS.

By Professor CLELAND, F.R.S., Galway.

[Professor Cleland considers that there is an utter misconception of the properties of saccharated lime, but in this paper he refers especially to its use in fevers.]

The two points most necessary to insist on in the administration of this remedy are probably large doses and copious dilution. A teaspoonful of the pharmacopœial “liquor,” given after meals, is necessary, in most cases, to develop its effects as a stomachic, and should be diluted in three or four ounces of water; but in cases in which there is foulness of the tongue from acute causes, much the best plan is to fill a tumbler with water, introduce as much of the solution of lime as can be added without giving a very disagreeable taste, and let the patient drink of the mixture *ad libitum*; and when one tumbler is finished let another be filled. Generally, I believe, in most cases in which there is great foulness of the tongue, this treatment will be found most advantageous. It is not suitable, however, when there is inflammation of the stomach, or great irritability indicated, for example, by central redness of the tongue, with sharp ragged edges. Attacks of bilious or gouty diarrhoea and British cholera yield with great rapidity to the free use of saccharated lime, and it is found exceedingly grateful by the patient on account of its alleviating the dry uncomfortable feeling in his mouth. It produces usually copious diuresis, a gentle perspiration, and relief from tormina, while the violence of the action of the bowels is quickly mitigated. There is never, however, the slightest constipation produced, the action of saccharated lime being slightly aperient, and even in some persons markedly so.

In fever, I do not claim any specific virtue for this remedy. Its action is in great part intelligible, and requires merely to be mentioned, I imagine, to commend itself to the profession as being highly advantageous. In many instances I have had

occasion to observe that when an hospital patient had a black, parched, and cracked tongue, like a cinder, with the teeth and lips black with sordes, saccharated lime being administered freely, the tongue was moist the following day, and in a few days the black crust had entirely loosened and disappeared. The cases to which I allude have all been cases of typhus, no typhoid cases having come into the Galway Fever Hospital during the past year, nor for some time previously. In these circumstances, although I have tried saccharated lime in typhoid fever, I am not prepared to make any statement with reference to its utility in that particular disease; but in any case of black parched tongue, no matter what the disease, I should rely on its effects. It is even beneficial where that symptom exists from urinary causes. In typhus it has become my habit to give the lime in milk instead of water. Given in this way it has the advantage of preventing that peculiar sensation in the mouth which is liable to follow the drinking of milk, and make it unsuitable for quenching thirst. Thus the patient is induced to take a certain amount of mild nutriment when he is most prostrate. It can scarcely be doubted that to this circumstance, as well as to the improvement in the whole length of the alimentary canal, indicated by the effect on the tongue, the benefits which I have observed to follow the free use of saccharated lime are to be imputed. I do not pretend to see a large number of cases; but those which I have seen have been sufficient to justify these remarks. Within the last year, of sixteen females admitted to hospital, suffering from typhus, and treated by me, only one died; and that one case is rather an example of the efficacy of the remedy of which I speak, than the reverse; for she was an aged woman with her tongue parched and blackened to a cinder, and within a few days of the fever running its course when she was admitted; yet she speedily revived, and when at last she succumbed, after several weeks, it was under the effects of exceedingly extensive bed-sores, which had commenced before her admission, and affected the pubis, nates, and both hips. In no case do I impute to the saccharated lime effects which might rather be considered due to stimulants; for, though convinced of the necessity of the latter in moderation, in hospital cases I have probably never ordered more than six ounces of wine, and four ounces of spirits *per diem*, and very rarely so much.—*Practitioner*, Dec. 1875, p. 401.

6.—ON FEVER.

By Dr. J. BURDON SANDERSON, LL.D., F.R.S.

The general conclusion to be derived from a whole series of cases is that in the early stage of fever a patient excretes about

three times as much urea as he would do on the same diet if he were in health, the difference between the fevered and the healthy body consisting chiefly in this, that whereas the former discharges a quantity of nitrogen equal to that taken in, the latter wastes the store of nitrogen contained in its own juices. That this disorder of nutrition is an essential constituent of the febrile process is indicated by the fact that it not only accompanies the other phenomena of fever during their whole course, but precedes the earliest symptoms and follows the latest. That it anticipates the beginning of fever was first demonstrated by Dr. Sidney Ringer in his investigation of the relation between temperature and the discharge of urea in ague. That the same condition continues after the crisis has past, *i.e.*, the temperature has begun to sink, was shown by Dr. Squarey in his investigation of eighteen cases of typhus, in all of which the daily excretion of urea was measured, and the variations of temperature were observed during the whole course of the disease, and the observations were continued until convalescence was completely established. In these cases it was found that, whereas the bodily temperature which in this disease rises rapidly at the beginning, and keeps up without sensible abatement during a period which often extends to the middle of the second week, usually begins to fall after the tenth day, the daily rate of discharge of urea, although usually above the normal during the first week, did not attain its maximum until the temperature had been falling for some days.

The question of the source from which the urea increment of fever comes is one which can be better discussed subsequently. At present it is sufficient to notice that the anticipation of the obvious symptoms of illness, particularly of the pyrexia, by the increased excretion of urea, as well as the continuance of the urea excess during the epicritical period, plainly indicate that pyrexia is not the agent by the direct influence of which the increased secretion of urea is produced.

Another consideration suggested by the same facts is this, that the mere increase of the per-centage of urea discharged affords an adequate measure of the waste of nitrogen, *i.e.*, of albumen, which actually occurs in fever; for to form a just estimate, the overlapping at both ends of the process ought clearly to be taken into account. Moreover, in fever there are very frequently losses of nitrogen by the bowels and skin, as well as by exudation, the amount of which scarcely admits of being determined.

It having been established that there is an increased discharge of nitrogen in fever, it remains to state what is known as to its source. There are two sources which are open to dis-

cussion, viz.: (1) the albumen of blood and lymph, and (2) that of the tissues; or, to use the expression which the researches of Voit have rendered current in physiology, store albumen, and tissue albumen. By the former we understand the albuminous constituents of the corpuscles and plasma as well as of the tissue juice or lymph; by the latter, the material of protoplasm, including that of the blood corpuscles.

Here the basis of observation is furnished by researches made by Dr. Salkowski, relating to the proportion of potassium salts discharged by the urine in fever, as compared with that of sodium salts. These researches relate to some twenty cases of various forms of febrile disease in Professor Leyden's wards at Königsberg. The research began with an investigation of the relative proportion of potassium and sodium salts discharged by the liquid and solid excreta in health, the observer being himself the subject of observation. The diet being mixed, and the nutritive condition nearly that of nitrogen equilibrium as seen by the constancy of the daily discharge of urea (min. 25.3, max. 27.2, mean of seven days 25.69), the daily quantity of potassium and sodium salts respectively, reckoned as potash and soda were: potash, 3.094 grammes, soda, 4.207 grammes; so that of the sum of both alkalies potash constituted 41.4 per cent.

In another individual, a clerk, on low diet without meat, affected with syphilis but in good general health, the soda discharge was about the same, but that of the potash much less, so that the potash per-centage varied from 18 to 26. From these and other observations it was concluded that the daily potash discharge of a healthy person on fever diet is less than one gramme.

The febrile cases investigated were one of relapsing fever, one of erysipelas, and several of pneumonia. In the case of relapsing fever, which was observed during part of the first paroxysm, the whole of the first remission, and of the first relapse and second remission, it was most distinctly seen, that whereas during the remission the potash per-centage of the total discharge of both alkalies sank to about 18.20, it rose during and especially after each crisis to about 90. In the case of erysipelas and in the pneumonia cases there was a corresponding relative and absolute increase of the potash discharge. There were, however, peculiarities in all the cases, which have been fully described by the authors, and are of sufficient importance to require notice.

On the whole the absolute quantity of potassium discharged on febrile days is three or four times as great as on non-febrile. As regards soda the results are entirely different. During fever it is seen in most of the tables that the soda discharge is

extremely low. As soon as the crisis is passed it at once begins to increase to such an extent that in one day as much soda is eliminated as on all the previous days taken together. Simultaneously the per-centage of potash discharge falls to its lowest.

The augmentation of potash discharge in fever, when little or no meat is being taken, and its rapid decline in defervescence, shows that the augmented production of urea in fever must take place at the expense of some source of albumen which contains potash. We have, therefore, in this fact an answer to the question from which we started. The albumen which serves as a source of urea in fever is not derived from liquor sanguinis (for the liquor sanguinis abounds in sodium salts, but contains very little potassium), but either from the blood corpuscles or from muscle, or both.

The very remarkable diminution of the discharge of sodium signifies of course that in fever, the common salt, which constitutes the bulk of the salts of the blood, is retained; for immediately after the crisis (as shown most distinctly in three of the cases), it passed into the urine in great abundance.

In addition to increased excretion of potash there is another circumstance which points to the blood corpuscles or to the muscular tissue as the chief seat of disintegration in fever, namely, the increased discharge of colouring matter. Unfortunately as regards this most important question sufficient information is wanting. There are, to the best of my knowledge, no comparative determinations either of the proportion of blood corpuscles or (what would be as useful) of the iron per-centage of the blood before and after acute fever, either in man or in the lower animals. The only facts relating to the subjects that I know of are (1), that in all febrile diseases the colouring matter of the urine, which is probably derived ultimately from the blood hæmoglobin, is three or four times as abundant as in health (*see Neubauer and Vogel*); and (2) that after traumatic fever in dogs there is a very marked diminution, both of the corpuscles and of the iron of the blood. But these observations are quite inadequate to serve as a basis for an opinion as to the proportion which the breaking-down of blood corpuscles bears to the total disintegration of fever. Of the many questions which require answering, there is perhaps none which is of greater importance, for if, as appears probable, the destruction of the coloured corpuscles is a part of the febrile process, the fact must have a very important bearing, not merely on the process itself, but on its after results. The colouring matter of the blood being the means by which oxygen is distributed to the tissues, the destruction of it must impair every function of organic life.

As regards the exhalation of carbonic acid in fever, the existing knowledge is much more unsatisfactory than that which relates to the disengagement of nitrogen. Indeed, the only observations on the subject are those of Prof. Leyden.

We are now in a position to take a general view of the febrile process, so far as relates to the exchange of material and the disintegration of tissue. We have seen that it is established on grounds which do not admit of any question, that a fevered man or animal discharges more nitrogen than a healthy person or animal on the same nitrogen income; and, that as regards man, the febrile excess amounts to something like three quarters of the normal expenditure. We have also seen that in man there is during fever an excess of discharge of carbonic acid, and that this cannot be accounted for as the mere result of excessive respiration, but that no such excess is observed in the dog. We have now to bring these facts into relation with each other.

In health, the whole of the nitrogen discharge is derived from food. In inanition, when nitrogen income vanishes, all the nitrogen which passes out as urea or otherwise is derived from stored or tissue albumen. In fever this is also the case, for the nitrogen income is as defective in the one condition as in the other; but besides the using up of stored albumen, there is an additional and altogether abnormal disintegration which, for reasons already stated, we believe to take place at the expense of blood corpuscles, of muscle or other tissue. That fact must be taken as a starting point in any attempt to understand the febrile process as a disorder of nutrition.

A healthy adult on ordinary mixed diet discharges from 30 to 36 grammes of urea daily; a healthy adult, on fever diet, discharges from 15 to 20 grammes; but if he is fevered he discharges, on the same diet, a little more than on ordinary diet in health, say from 35 to 40 grammes. A normal person on fever diet discharges about 22 grammes in an hour, of carbonic acid, in health; about 32·3 grammes in fever. Where does this come from?—To account for the urea excess, one must suppose (the nitrogen equivalent of one gramme of urea being 3 grammes of albumen) that 60 grammes of albumen is disintegrated in 24 hours. These 60 grammes would (the carbon per-centage of albumen being 53) contain 31·8 grammes of carbon, of which one-fifth of the weight of urea discharge (the proportion of carbon in any given weight of urea being $\frac{12}{60} = \frac{1}{5}$), *i.e.*, four grammes, would pass out as urea. The remainder, *viz.*, 27·8 grammes of carbon correspond to 102 grammes of carbonic acid, which would therefore express the increase of carbonic acid discharge which would result, supposing that all the carbon of the tissue disintegrated to form the febrile excess

of urea left the body in that form. 102 grammes per day is only $4\frac{1}{4}$ grammes per hour; consequently the quantity of carbonic acid produced by the disintegration of albumen in fever is a mere fraction of the total quantity exhaled (32.3 grammes). We have therefore a remainder of 28 grammes which must be derived from the consumption in the body of material not containing nitrogen.—*Practitioner*, April, 1876, p. 272.

7.—COLD WATER IN FEVER.

By Dr. C. BINZ, Professor at the University of Bonn.

It is not long since every fever patient was carefully guarded from pure air and fresh water. Thick blankets and hot beverages seemed indispensable. Several medical men observed that this treatment did more harm than good; but James Currie was the first to have any success in fighting against these prejudices.

Old and deeply-rooted errors do not fall at one blow, especially when they derive support from the extravagances of the opposition. So at least it was with us. Priessnitz, and the fanatic hydrotherapeutists who followed him, barred the way for a long time to the rational use of cool water; and it is only about fifteen years ago that we recurred to the healthy principles of Currie.

The matter itself is very simple. If a patient at 40° C. (104 F.) is placed in a bath at a lower temperature, he must quickly part with heat. In fever, the natural regulation of heat which keeps our body at an almost equal temperature is insufficient. The cool bath makes up for this. If we measure the temperature after the bath, we shall find it lower than before. The blood that surrounds the cells of our nerve centres is less hot. The patient therefore feels stronger and quieter.

Cold baths (15-20° C. = 60-68 F.) have the clearest effect. Extensive experience has taught that their action is most positive when they are short and often repeated. Very weak patients must begin with 35° (97 F.), and then the warmth must be lowered to 20° (68 F.), by carefully and gradually adding cold water. In the meantime the body should be gently rubbed.

Cold sheets (*Kälte Einwicklungen*) are less efficacious and cold affusions (*Uebergiessungen*) have less effect; this latter considered merely from the antipyretic point of view.

Only a high degree of weakness of the heart, loss of blood or perforation of the bowels, are contra-indications against the use of cool baths. Menstruation is not one when the fever is at a dangerous height, and pregnancy never. Every age and

every constitution permits the withdrawal of fever heat, only it must be observed that the loss of heat is in inverse proportion to the weight of the body. For babies we need therefore seldom go under 30° C. (86° F.) to have a full effect; the temperature of the water must be lowest for strong adults.

External application of cold proves, like all other febrifuges, to be most efficacious when the temperature has a tendency to sink spontaneously. That is from seven in the evening till morning, and again in the day from eleven till two o'clock.

The after-effect is of great importance. Under some circumstances it lasts several hours; that is, the lower temperature continues even when the patient has been removed from the water. The reason is probably as follows:—

In fever the vessels of the skin are generally much contracted. The cool water acts as a strong stimulant on them, and causes a somewhat stronger contraction to take place, but this is only of short duration. Relaxation for a longer term is the necessary consequence. The hitherto bloodless and dry skin becomes filled and moist, and thus the irradiation of warmth goes on. It is easy to convince oneself of this state of the skin after the bath. The cooler the bath and the longer it lasts, the more evident and the more lasting will be its result.—*Practitioner*, April, 1876, p. 282.

8.—PERNICIOUS FEVER: FEBRIS ALGIDA AND FEBRIS COMATOSA.

By Dr. JOHN SULLIVAN, Havana, Cuba.

It may easily be imagined that the poison of malaria, in proportion to the degree of its intensity or destructive power to the susceptibility or the bodily condition of the recipient of such poison, may generate a fever, which from its tendency to paralyse all nerve-power, and to promote a dissolution of the elements of the blood, may justly be denominated “pernicious fever.”

No poison has ever introduced itself into the human system so capable of producing effects so varied, of assuming such questionable shapes, of wearing so many masks and disguises, as the poison of marsh fever,—effects which vary in degree from a slight depression of nerve-power to a partial extinction of sense, and even to the complete destruction of life. But the poison of malaria will often lay concealed in the system like a coiled-up serpent, ready at any moment to dart its venom to complicate and exaggerate any disease to which the body may be subject, if it do not completely control and subject such body to its undivided influence; or it will take upon it so many Protean forms that its victim will be thrown off his

guard, and probably die from its masked effects. But under no form does malaria show itself with so great intensity as under the form of pernicious fever. This fever may appear under the form of algid or of comatose fever, with all their varied complications—*febris algida* being the most complete expression of the true nature of the poison, paralysing all nerve-power, determining the blood to internal organs, and thus producing that intense alidity as of death; while *febris comatosa* is marked by an especial malarious determination to the brain, producing an anæmic contraction of arteries as in a fit of epilepsy.

It sometimes happens, while attending upon a case of intermittent fever, when we imagine that the case is progressing satisfactorily and with all due regularity, on a sudden we observe the symptoms to alter greatly for the worse: one or more of its stages may become present in great intensity, complicating or placing in jeopardy one or more of the organs essential to life. In the tropics, pernicious fever may be seen in all its most formidable array, complicating the various types of intermittent, and tending to aggravate a variety of morbid states which have no necessary connexion with the poison of malaria. The prognosis must depend upon several conditions. Suppression of urine is an alarming symptom, but should the secretion be restored, an improvement is sure to follow. Convulsions, frequent in children, are, perhaps, next to black vomit, the most dangerous symptom; when they make their appearance at the close of an attack of fever, there is always danger to life. Vomiting of black matter is fatal to adults; I have seen children occasionally recover.

There are two symptoms which, when they do occur in the course of an intermittent, always occasion me great alarm and anxiety. The one is an acute pain which fixes itself in some part of the body—sometimes in the side, like an attack of pleurisy, at others it strikes upon the hip-joint. Now, from long experience I have always found the accession of these pains to be a very ominous symptom. The second symptom, of still rarer occurrence, is the vomiting of blue (not black) matter; it may occur in the adult who has suffered from marsh cachexia, or from chronic diarrhœa.

Pernicious fever, the reverse of yellow fever, may repeat itself. I have known persons whose life was always a burden to them from the fear of a fresh attack. All races suffer from it. Old people often die from it. I have seen children attacked by it two or three times almost imperceptibly, then begin to vomit up a blackish fluid, followed by convulsions and death.

In children, when fever is ushered in by general or partial

convulsions, the symptom is generally fatal, as indicating a tendency to the same manifestation in every succeeding attack.

All chronic diseases in tropical climates, all deep-seated causes of debility, privation, and suffering, the emotions from grief or anxiety, alcoholism, predispose to attacks of pernicious fever, since malaria directly attacks nerve-power, and when this has been shattered from the above causes, it becomes more sensitive to its influence. The varieties of pernicious fever characterised by some violent predominant symptom may be classed into two principal groups—*febris algida* comprising the fainting, dysenteric and choleraic varieties; *febris comatosa*, the tetanic, lethargic and convulsive.

Algid Pernicious Fever.—*Febris algida* would appear to be the most faithful manifestation of the *modus operandi* of the poison of malaria on the living organism—perhaps the only pathological base upon which all other pernicious fevers rest, so that it will sometimes appear in its naked uncomplicated form as a termination of pernicious fever.

The progress of algidity is usually insidious; it often shows itself in the middle of an apparently simple attack of intermittent. The patient appears in his usual health; presently the face turns pale, the features become sharpened and drawn in, the lips turn blue, the eyes hollow, the pupils become dilated, the pallor and coldness gradually go on increasing, and cold, clammy sweats pervade the entire body; you might fancy you were gazing on a corpse. The hands and feet first turn cold, next the extremities; the abdomen alone preserves some heat. If you touch the skin it feels like marble, and yet the patient complains of a sensation of heat internally. The tongue is the last to become cold, and when the air expired is colder than that inspired, the danger becomes imminent. The urine is often suppressed as the algidity increases; the skin becomes more and more bathed in a cold perspiration. The heart beats slowly and is scarcely audible; the pulse thready, not to be felt at the wrist. The intellect is unimpaired. The patient does not complain of any particular pain. Respiration, from being calm and slow, becomes anxious and rapid. To a calm succeeds a vague sense of uneasiness, which is soon replaced by a feeling of approaching death.

The most remarkable case of algid pernicious fever that I ever witnessed was the following, which I will now relate. In the present year (April, 1875) I was called to visit the wife of D. B. M., a well-known sugar planter in Cuba. She had been delivered of a child about two months before, at which time she complained of slight chills and fever; since then severe intermittent had set in. I was greatly surprised on my arrival at the consternation and dismay depicted on the countenances

of the patient's friends and attendants. I decided upon taking a view of the patient unseen by her. She was seated in an arm-chair, the arms of which she firmly grasped with both hands; the body slightly inclined forwards, rigid, erect; the face was pale and cadaverous, the countenance expressive of anxiety and terror, mixed with a certain degree of sadness. She looked as it were on vacancy, and took no notice of those who surrounded her. On my entering the patient's room, she directed an anxious and enquiring look towards me, as if conscious who I was, and desirous to be relieved. I took her hand, which, as well as the entire body, was bathed in a cold, clammy, icy sweat; no pulse at wrist. She could only reply to my questions in a whisper, but a whisper deep, cold, and unearthly. The heart's beat was strong and even tumultuous, as though labouring hard to upheave and dislodge the mass of blood by which it was oppressed. The right heart communicated its impulse to the jugular veins,—hence strong venous pulsation.

I found, on examination, considerable congestion and enlargement of the spleen, and induration of an atrophied liver. I ordered a hot mustard foot-bath, ten grains of calomel, with fifteen grains of quinine, to be taken immediately,—the dose to be repeated in two hours' time; frictions of quinine in brandy (two to six drachms) to be rubbed over the extremities and down the spine every half-hour. About four hours after my first visit the heat of the body returned, and the patient was enabled to lie down in her bed, which she had not been able to do during the last three weeks. After the second dose, I followed up the calomel and quinine in smaller proportions—two grains of calomel to four of quinine every four hours. I ordered besides, in order to promote the partially suspended secretion of urine, two tablespoonfuls of a strong diuretic mixture (every two hours, and placed over the loins (a custom peculiar to the country people) some digitalis leaves wetted with vinegar. After eight hours had elapsed, considerable relief was obtained and all the symptoms improved; the secretion of urine was restored, circulation became less impeded, and some sleep was procured. The algidity returned daily about 3 p.m. for three successive days, but in a gradually milder form, of less duration; and the patient finally completely recovered. This case I regard as very instructive and interesting, as it is a complete manifestation of the effect of malaria upon the human body. It would seem that the miasmatic poison availed itself of the new inlet into the system by the medium of the raw and dilated surface of the vagina and uterus after delivery—a medium through which, under similar circumstances, other septic poisons have been introduced, and

with fatal effect, into the system. This intensified dose of malaria would appear to have generated algid pernicious fever. Large doses of quinine in this case proved most triumphantly its value and its nature as an antiseptic.

The diagnosis is easy when the pernicious fever is simple, but when complicated with dysentery or cholera it is rather obscure. Between pernicious choleraic fever and Asiatic cholera it is almost impossible to diagnose, especially when the two endemics exist at one and the same time. But between simple dysentery and pernicious dysenteric fever the diagnosis is more easy. The stools are not so bloody; they are more abundant and liquid, and provoke less tenesmus. The antecedents—attacks of fever, algidity, and cold sweats, &c.—make the diagnosis more easy. We must not confound the fever of pernicious dysenteric fever with that which accompanies simple or true dysentery, which is symptomatic; while the fever of pernicious dysenteric fever, being of malarious origin, is independent—a foreign element superadded to the principal malady, which it aggravates and often renders mortal. This is an important fact, worthy to be remembered in our practice in tropical climates. Hence, in the complicated dysentery, all remedies directed to the cure of dysentery will prove useless, so long as we do not neutralise the foreign morbid element by large doses of an antiseptic.

The attack of febris algida consists of one long stage. The cold increases gradually, with cold and clammy sweats; the voice falls to a whisper; the pulse falters, finally ceases; and the patient dies in a swoon. Should reaction take place, the pulse rallies; the heat returns gradually to the surface from the abdomen, in which it would appear to have concentrated itself, radiates to the chest, and thence to the whole body. The cold stage, or state of algidity, may last forty-eight hours or longer. Recovery is often insidious, and may be complicated with symptoms of an ataxic or typhoid nature. The mortality from febris algida is not so great as that from febris comatosa. In the algid state the powers of absorption do not appear to be annihilated, as in the cold access of Asiatic cholera.

Comatose Pernicious Fever.—Insolation is occasionally a most powerful cause. A man who leaves a marshy district, and who travels a long way exposed to a burning sun, is very subject to an attack of malarious apoplexy. The fit may seize him in the evening when the sun has set, or even at night-time when in bed. Like other pernicious fevers, the comatose form especially attacks those who, after a long residence in a marshy district, remove into another free from malaria.

The comatose form is usually preceded by some derangement

of the health—by two or three irregular attacks of fever, which may be trivial or scarcely perceptible.

The invasion of coma is violent, its progress rapid, its stages of intense severity; if the patient be seized when standing, he falls down as if struck by lightning; if when lying down, it comes on unperceived, and only to be revealed by his stertorous breathing.

I have never known delirium or convulsions precede an attack of pernicious malarious coma. I remember one patient, who had recovered, told me that he had experienced during two or three days before the attack a creeping sensation up the back, with a sense of suffocation and constriction about the chest. The cold feeling is absent, but the hot stage is soon developed; and, unlike the skin in febris alvida, it is covered with a hot perspiration—a frank intermittent is seldom observed. The fit may last from four to five hours, or even to as many days. The characteristic symptoms are: loss of motion, sensation, and intelligence, with impaired function of respiration and circulation; the patient answers pettishly and in monosyllables; the pupils are insensible to light; some low murmurs of complaint may be often heard, indicating severe headache. The frequent return of motion proves the intermittent character, when the symptoms generally improve. But in cases which threaten to end fatally, we often observe muscular contractions, as trismus, strabismus (complete abolition of sense and motion). There may be violent tonic and clonic contractions; and death often closes the scene, after one or two attacks of black vomit.

The diagnosis is not difficult. The antecedents—previous attacks of intermittent, the paludinous diathesis—the absence of all plethoric symptoms, will clear the way for a correct diagnosis. The only difficulty is to diagnose between the apoplectic form of pernicious fever and true apoplexy. This occurs but very rarely in the tropics, but may occur to strangers who have lately arrived. We must be careful not to bleed in pernicious apoplexy, as distinct from true apoplexy. Bleeding would inevitably hasten the fatal termination. By not interfering too hastily, by waiting a little, by adopting the maxim *festina lente*, so as to allow the disease to clear up, we may be enabled to form a just diagnosis. The heat and moisture of the skin, the hurried pulse, indicate that the apoplectic fit derives its origin from marsh poison. Post-mortem examination reveals no inflammation of the brain or of its membranes, nor any lesion to which might be traced the severity of the symptoms. We may find, as in all fevers of a certain duration, the membranes, especially the pia mater,

somewhat injected; the cerebral substance redder than usual; some serum infused into the ventricles; and when black vomit has occurred, we shall find the mucous membrane of the stomach of a yellowish tint, streaked with violet-coloured stains.

The almost impossibility of administering proper medicines adds to the danger of this form of pernicious fever. Quinine can be introduced into the stomach with difficulty, by reason of the spasm of the pharynx or œsophagus. We must, therefore, have recourse to injections of the drug per rectum, to frictions of quinine in alcoholic solution so as to produce absorption. In these cases the subcutaneous injection of quinine is a valuable method of its introduction into the system. It possesses the advantage of economy, is easily applied however severe may be the vomiting, difficulty of swallowing, or coma.

Mr. W. Moore, of Bombay, has tried the hypodermic method on a large scale. He assures us that he has injected quinine successfully in 100 or more cases of intermittent and remittent fevers. The quinine must be used in solution, not in suspension. The best time to inject is before the cold stage.

Several French, German, and American physicians have advocated the subcutaneous method in all cases of intermittent, especially where quinine cannot be administered by mouth. I cannot, from my own experience, bear witness to the great advantages to be derived from the subcutaneous method. I have tried the plan eleven times, three times with a certain beneficial result; the effect was always a shorter duration than when quinine was introduced by any other method, and I had to renew the injection frequently. I would have injected more frequently had I not been deterred in my practice by the inconvenience from little accidents arising from the puncture made by the injecting-syringe, some swellings, and often superficial abscesses, which were looked upon by my Spanish patients in Cuba with a most ridiculous degree of anxiety. For my part, I have trusted more, in the treatment of febris comatosa, to revulsives—hot mustard foot-bath, blisters to the calves of the legs, strong injections of quinine per rectum, having previously administered a purgative enema, strong frictions of quinine, etc. It is seldom that this plan fails to develop the malarious origin of this comatose form of pernicious fever; but in no case, however strong the indication may appear, however intense the symptoms may be, must we bleed. A couple of leeches may be applied over the mastoid process. When the tendency towards convalescence becomes established, should speech appear to be somewhat embarrassed, should we

observe dulness and uncertainty in the gait, in such cases I have always derived advantage from the combination of calomel and quinine.

Let us consider, before we conclude, why the poison of malaria in a pernicious or highly destructive form produces intense cold (*febris algida*) in one case, and intense heat (*febris comatosa*) in the other.

The system of the great sympathetic presides over organic life. It brings into intimate relation and connexion and into sympathetic harmony all, and even the most distant, parts of the human system; and it is through its medium that all impressions and emotions, whether moral or physical, are conveyed, transmitted, and manifested. When this system of nerve sympathy is impressed by the incubus of the miasmatic poison, independently of the brain, a profoundly depressing and paralysing effect is produced as the consequence. This impression is conveyed to the arteries, even to the remotest capillaries, the calibre of which is regulated by the sympathetic, causing—first, dilatation; next, contraction, and consequent obstruction to the circulation. Hence the gradual retrocession of the blood from the extremities; hence the intense cold, and cold sweats. Still, the intellect and the functions of the brain remain unimpaired; and thus is produced that form of malarious pernicious fever, *febris algida*. But supposing that, owing to certain conditions of the human body, the poison, instead of acting immediately upon the sympathetic system of nerves, should determine violently to the brain, when the body is in the full enjoyment of its natural heat, and in the performance of all its functions: in such a case the body is, as it were, stormed and taken by surprise.

The functions of the brain preside over animal, rather than organic, life; it directs—does not perform—the functions of the great sympathetic, through whose agency life and heat are carried on independently of the brain. We know that animals are sometimes born with a great deficiency or imperfection of brain or cerebral matter; and, nevertheless, life and heat are carried on independently of the brain for a longer or shorter period of time. When, therefore, the function of the brain is suddenly overpowered and paralysed by the baleful incubus of marsh malaria, we have loss of motion and sensation; but the organic functions continue to work on, and the heat of the body—even greater than natural—is kept up to the last, until the patient recovers from the fit, or life becomes extinct. And this constitutes the comatose form of pernicious fever of malarious origin.—*Medical Times and Gazette*, March 11, 1876, p. 277.

9.—ON SALICYLIC ACID AS AN ANTIPYRETIC.

By Dr. C. A. EWALD, Assistant Physician to Prof. Frerich's Wards in the Charité Hospital, Berlin.

[Dr. Ewald has had experience of the use of salicylic acid in upwards of 100 cases, in which from 300 to 350 single doses have been administered.]

1. *Preparation and Dose.*—It is a matter of indifference whether the pure acid or one of its salts, best the sodium salt, be used. As, however, impure preparations, containing carbolic acid, &c., have a strong astringent and even caustic action, and since, in all probability, the acid undergoes conversion into the sodium salt in the blood, it is better to use the sodium salts at once. Whether, however, the acid or its salt be employed, it is necessary that each dose should be a large one, as small or divided doses have little or no effect. Generally speaking, the minimum dose required to reduce the temperature effectually is 5·0 grammes (77 grains). Should such a dose have no effect, one may after four or five hours with safety give a second, or indeed even a third similar dose. I have seen cases in which 15 or 20 grammes of the salt have been given during twenty-four hours without any ill effect. The salt is readily soluble in water, and is taken by most patients without complaint. In those rare cases in which malaise or vomiting occurred three or four drops of chloroform sufficed to remove them.

2. *Action on the Temperature.*—As a matter of course, the effect of the salicylic acid was not the same in each case, but it has proved to be more trustworthy and efficacious in its action than all other antipyretics. I have collected the results of one hundred single doses in cases of typhoid of nearly equal severity. In order to test the value of the drug more accurately, it was always given at midday, before the usual afternoon rise of temperature. Almost immediately, the temperature began to fall, the maximum result being reached, in most cases, from four to five hours after the administration of the medicine, but in some cases not less than eighteen to twenty hours were required. The subsequent rise of temperature was as gradual as its fall, but in mild cases it never reached the same height as before. These facts were established in many cases by thermometric observations made every ten minutes in the axilla and rectum. The following are the results obtained in this way, which have been confirmed by further experience. Where the drug had an effect the maximum fall, after its administration, was $4\cdot3^{\circ}$ C., where it had none, the maximum rise was $1\cdot5^{\circ}$ C. The temperature at 5 o'clock on those days on which, at 12 o'clock, five grammes of salicylic acid had been given, was more than one degree lower than the temperature at 8 o'clock in the morning of the

same day, in 45 per cent., and less than one degree in 35 per cent. of the cases. On the whole, then, a reduction in temperature was observed in 80 per cent. The temperature was increased in 20 per cent., and in four cases the increase exceeded 1° C. These results prove the superiority of salicylic acid over all other known antipyretics.

3. *General Action.*—Within fifteen minutes, or even less, after the administration, a copious respiration breaks out, first on the face, then on the thorax, abdomen, and the rest of the body, accompanied by reddening of the skin, more especially of that of the face, and may be so copious that the patients may lose 500-750 grammes of water. Almost simultaneously with the outbreak of this sweating, sometimes a little later, the temperature begins to decline, the gradual fall lasting much longer than the perspiration. Nor is there, indeed, any constant relation between the fall of temperature and the amount of sweating, there being in many cases great reduction of temperature with little or no sweating. Generally the pulse and respiration are not at all affected, though the pulse may become a little slower. Where the pure acid or the sodium salt are employed, the intestinal tract does not appear to be at all affected. The recorded accounts of irritant effects on the mucous membrane of the oesophagus, stomach or intestines, even of erosions and hemorrhages, are due to admixture of irritant substances, such as carbolic acid, with the salicylic acid. This is shown by a comparison between my earlier experience with my later, as I have not met with any such results since using the pure drug. The evacuations, on the other hand, become more frequent and fluid. The salicylic acid appears in the urine, which is otherwise unaltered, as salicyluric acid. The cerebral functions appear little or not at all interfered with, for so far as my own experience goes, only three patients have complained of buzzing in the ears and dizziness, and only one of hallucinations. Nor does such a collapse occur as one might have expected from the great fall of temperature. I have not, nor indeed have any others, ever seen a fatal case of collapse, and although several patients, especially such as have sweated profusely, appear during the fall of the temperature or shortly afterwards much exhausted and very pale, this condition is at most rare and transitory. As it is quite possible that owing to so great a reduction of the heat of the body, a fatal collapse may occur, I have not been in the habit of giving the acid to very debilitated patients, or when I have done so I have at the same time administered analeptic and stimulating remedies.—*Practitioner*, March, 1876, p. 201.

10.—THE TREATMENT OF RHEUMATIC FEVER WITH SALICYLIC ACID.

By the EDITOR of the MEDICAL TIMES AND GAZETTE.

Like all new remedies, salicylic acid is being tested in a number of different diseases, and among these of course rheumatic fever occupies a prominent place, not only on account of its frequency, but also of its markedly febrile character. The results which have been obtained in Prof. Traube's wards at Berlin certainly afford not a little encouragement to indulgence in a hope that in salicylic acid a real remedy for acute rheumatism has at last been found. We extract the following details from a paper by Staff-Surgeon Stricker, in the *Berliner Klinische Wochenschrift* of January 3, 1876. For several months all the cases of acute rheumatism in which the local symptoms were strongly marked (fourteen in all) were treated with salicylic acid. The preparation used was, however, not the ordinary impure commercial acid, but one that by repeated crystallisation had been rendered almost perfectly pure. Thus prepared it consists of shining white needles, which have no smell, and dissolve completely in water and alcohol so as to form a clear solution. This pure acid can be given internally in considerable doses without any of those unpleasant results which have followed the use of the commercial acid, which probably owes its caustic properties to the presence of other substances—for instance, carbolic acid. The pure acid only excites some dryness in the mucous membrane of the mouth and pharynx, followed by an increased secretion from their surfaces. This inconvenience can, however, be obviated by giving the acid in half-gramme or gramme doses every hour, in the form of powder, and enclosed in a capsule; and in the treatment of rheumatic fever the administration is continued until the joints which were previously affected can be moved without pain. To quote Dr. Stricker's words, "All the patients thus treated were not only relieved of their fever, but also of the local symptoms—*i.e.*, the swelling, redness, and especially the painfulness of their joints,—within forty-eight hours; most of them even within a much shorter period." The largest quantity of pure salicylic acid which was found necessary to produce this effect was fifteen grammes, and the smallest five grammes; but that even larger quantities can be taken internally without injuring the digestive apparatus is proved by the fact that one patient actually took twenty-two grammes in the course of twelve hours through an excess of zeal on his own part; but, nevertheless, his tongue became clean and his appetite returned in the course of this vigorous drugging. As far as Dr. Stricker's observations go, the more acute the case

the better the action of the acid. He finds it best to begin the treatment in the morning, for then its effects are generally so decided by the evening that it is unnecessary to disturb the patient's rest to give him his medicine. The general phenomena which were observed to follow large doses of the acid were copious perspiration, ringing in the ears, and slight deafness, and in two cases the patients became more than usually lively. Dr. Stricker does not pretend to express any opinion at present on the effect exerted by the acid on the cardiac complications of rheumatic fever. Most of his cases either had old valvular disease or else were suffering from a recent endocarditis at the time of their admission. The details of five cases are appended to Dr. Stricker's paper, and the temperature-sheets are given in graphic form in four of them, and we can only say that they confirm in a most striking manner what has been above stated as to the value of the salicylic treatment.

A further testimony in the same direction is that of Dr. L. Reiss (Berl. Klin. Wochenschrift, December 20, 1875, "On the Internal Use of Salicylic Acid.") He speaks of the results obtained in acute rheumatism as unexpectedly favourable, and points out that the action of the salicylic acid must be more than purely symptomatic, since in some cases a single dose not only permanently reduced the temperature, but also was followed by general improvement in the patient's condition. The large opportunities which our London hospitals afford for the observation of rheumatic fever should certainly be utilised for the further investigation of the value of salicylic acid. We again remind our readers that the acid used must be chemically pure. (*Pure salicylic acid can be obtained from the Chemische Fabrik auf Actien (vormals E. Schering), Berlin, N. Feunstrasse 4.*)—*Medical Times and Gazette*, Feb. 5, 1876, p. 144.

11.—TREATMENT OF ACUTE RHEUMATISM BY SALICIN.

By Dr. T. MACLAGAN, Dundee.

[The writer has tried all the usual remedies for acute rheumatism over and over again, and finds salicin to give better results than any.]

In the course of an investigation into the causation and pathology of acute febrile ailments, which has for some time engaged my attention, I was led to give some consideration to intermittent and to rheumatic fever. The more I studied these ailments the more was I struck with the points of analogy which existed between them. On a detailed consideration of these, I shall not now enter. Suffice it to say that they were sufficiently marked to lead me to regard rheumatic fever as being, in its pathology, more closely allied to intermittent

fever than to any other disease, an opinion which further reflection and extended experience have served only to strengthen.

Rheumatic fever is now-a-days generally regarded as being produced by some cause or agency which is generated within the body. My own investigations into its pathology have led me to reject this view, and to adopt the old "miasmatic" view of its mode of origin, according to which the cause which gives rise to the disease is introduced into the system from without.

Holding this view as to the pathology of rheumatic fever, impressed with the points of resemblance between it and intermittent fever, and bearing in mind that we have in quinine a potent remedy against the latter, there seemed to me good reason for indulging the hope that some remedy would yet be discovered capable of exercising a similar, if not equally beneficial action on rheumatic fever.

In reference to the action of quinine on the various forms of intermittent and remittent fever, and, indeed, with reference to the action of the Chinchonaceæ generally on the diseases of tropical climates (ipecacuanha in dysentery, for instance), there is one fact which has always strongly impressed me—the fact, namely, that the maladies on whose course they exercise the most beneficial action are most prevalent in those countries in which the Chinchonaceæ grow most readily; nature seeming to produce the remedy under climatic conditions similar to those which give rise to the disease.

Impressed with this fact, and believing in the miasmatic origin of rheumatic fever, it seemed to me that a remedy for that disease would most hopefully be looked for among those plants and trees whose favourite habitat presented conditions analogous to those under which the rheumatic miasm seemed most to prevail. A low-lying, damp locality, with a cold, rather than warm, climate, give the conditions under which rheumatic fever is most readily produced. On reflection, it seemed to me that the plants whose haunts best corresponded to such a description were those belonging to the natural order Salicaceæ, the various forms of willow. Among the Salicaceæ, therefore, I determined to search for a remedy for acute rheumatism. The bark of many species of willow contains a bitter principle called salicin. This principle was exactly what I wanted: to it, therefore, I determined to have recourse. It will thus be seen that the employment of salicin in the treatment of acute rheumatism was no haphazard experiment, but had a fair foundation in reason and analogy.

Salicin has long enjoyed a reputation for tonic and febrifuge properties, and was at one time a good deal used as a substi-

tute for quinine. It has of late years, however, gone very much out of use, and now it does not even find a place in the British Pharmacopœia.

The idea of treating acute rheumatism by salicin occurred to me in November, 1874. I had at the time under my care a well-marked case of the disease (Case 1) which was being treated by alkalies, but was not improving. I determined to give salicin; but before doing so, took myself first five, then ten, and then thirty grains without experiencing the least inconvenience or discomfort. Satisfied as to the safety of its administration, I gave to the patient referred to twelve grains every three hours. The result exceeded my most sanguine expectations. For some days prior to its administration the temperature had ranged from 101.8° to 103° ; the pulse was 120, and the joints were swollen and very painful. On the 26th of November the alkaline treatment was stopped, and that by salicin commenced. On the following day, after eighty-four grains of salicin had been taken, the pulse had gone down to 100, the temperature to 99.6° (from 102.8° the previous day), a fall of over 3° , the pain and swelling of joints, but especially the pain, had much abated, the joints could be moved a little, and the patient expressed himself as being much better. On the next day (Nov. 28th) the temperature was natural and the pain all but gone, the joints still remaining stiff. From this time he convalesced steadily and quickly.

The case was a very striking one; but, by itself, could not be regarded as proof of the beneficial action of salicin. I was quite aware that cases of acute rheumatism do sometimes unexpectedly improve without any treatment, and had no surety that this was not a case in point. It afforded me, however, strong encouragement to persevere with the salicin. This I did; and all the cases of acute and subacute, and several cases of chronic, rheumatism which have come under my care since then have been treated by this remedy, and with results much more satisfactory than I ever got from any other remedy,—the results being most marked and most satisfactory in distinctly acute cases, and least so in chronic cases. Subjoined are the details of three cases.

Case 1.—Acute Rheumatism.—William R., aged forty-eight, was first seen on Nov. 24th, 1874. Had rheumatic fever eight years ago; was then confined to bed for eight weeks. With that exception has always enjoyed good health. Present illness commenced three days ago with shivering and pains in joints, which have increased in severity.

Nov. 24th. Has anxious, pained expression. Lies on his back without power of motion, the least movement causing intense pain. Skin covered with acid perspiration; tongue

moist and furred; bowels moved by medicine; urine scanty and high-coloured; pulse 120, small and regular; temperature 101.8° ; heart's sounds normal. To have twenty grains of acetate of potass every four hours, and ten grains of Dover's powder at bedtime. Food to consist of milk, beef-tea, and light puddings.

25th. Passed an almost sleepless night. General state unchanged. Has great pain in the joints, especially in the knees, ankles, wrists, and fingers, which are all a good deal swollen. Cannot move. Pulse 120, feeble; temperature 103° ; heart's sounds a little muffled; skin bathed in acid perspiration. Continue treatment.

26th. Had an hour's troubled sleep after the Dover's powder. No change in general condition; lies on his back, quite unable to move; profuse perspiration; pulse 120, feeble; heart-sounds indistinct; temperature, 102.8° . Omit potass and Dover's powder; to have twelve grains of salicin every three hours.

27th. Had four powders (forty-eight grains) before bedtime yesterday. Passed a much better night; slept for several hours in snatches of an hour at a time. Expresses himself as feeling much better, and looks so; says the powders did him a deal of good; can move his limbs a little, but not without pain; joints less swollen; skin covered with acid perspiration; tongue furred; bowels not moved; pulse 100, of better volume, soft and compressible; heart-sounds clearer; temperature 99.6° . Has had eighty-four grains of salicin. To continue it.

28th. Had a pretty good night; pain nearly gone, though still felt on moving the limbs; joints almost natural in size, except those of the fingers, which are still swollen; skin not perspiring so freely; secretion still acid; tongue cleaning; bowels not moved. Pulse 84, of good volume and character; heart-sounds distinct and normal; temperature 98.5° . To have a dose of castor oil, and to continue salicin every four hours.

20th. Passed a good night; pains quite gone, and can move the joints freely. Is loud in his praise of the powders, every one of which, he says, he felt do him good; wishes to continue them. Pulse 72; temperature 98.3° .

From this time convalescence was steady and satisfactory.

Case 2. — *Acute Rheumatism*.—Mrs. B., aged thirty-three. First seen on Dec. 18th, 1874. On Dec. 15th was seized with pains in hips and knees, for which she was compelled to go to bed. Had been shivering and feeling out of sorts for two or three days before.

Dec. 18th. Complains of pain in both knees, and in left wrist, which are all swollen and tender; skin covered with acid

perspiration; tongue furred; bowels moved by medicine; urine scanty and high-coloured; pulse 116; temperature 101.6° . To have twelve grains of salicin every four hours.

19th. Not much sleep; general condition unchanged. Pulse 116; temperature 102.2° . To take a powder every three hours.

20th. Better night; feels much better; can move knees without pain, though joints are still stiff; is quite free from pain; tongue cleaner; skin moist. Pulse 84; temperature 99° .

21st. Feels quite well; only a little stiff. Pulse 72; temperature 98.5° .

Convalesced satisfactorily.

Case 3.—Acute Rheumatism.—Henry B., aged thirty-four, had rheumatic fever seven years ago; was ill at that time for six or seven weeks.

March 14th. Two days ago felt generally out of sorts; at night felt cold and shivering and had aching pains in limbs, especially in hip-joints. Complains now of pain in right shoulder, both ankles, and left knee, the ankles and shoulder being most painful; all the affected joints are slightly swollen. Feels just as he did when his fever came on seven years ago. Has anxious expression; skin covered with acid perspiration; tongue thickly furred; bowels moved by medicine. Pulse 112; temperature 102.1° ; heart's sounds normal. To have twenty grains of salicin every three hours.

15th. Rather restless night; ankles and shoulder not so painful, but left knee more so; through some mistake did not have the powders during the night; free acid perspiration; pulse 120; temperature 101° . Continue salicin; heart's sounds normal.—Evening: Feels better; says the powders are giving him much relief. Pulse 100; temperature 99.3° . Continue salicin.

16th. Passed a much better night; skin moist; tongue less furred; expression much improved. Pulse 80; temperature 98.6° . All the joints free from pain, though stiff and giving slight pain on motion.

17th. Feels quite well. Pulse 68; temperature 98.2° .

From so small an experience of salicin as I have had, I would not assert in anything like a dogmatic manner the full extent of its usefulness. I would simply indicate the following conclusions as those to which I have been led, and which, I hope, a more extended experience of its use may confirm.

1. We have in salicin a valuable remedy in the treatment of acute rheumatism.

2. The more acute the case, the more marked the benefit produced.

3. In acute cases, its beneficial action is generally apparent

within 24, always within 48, hours of its administration in sufficient dose.

4. Given thus at the commencement of the attack, it seems sometimes to arrest the course of the malady as effectively as quinine cures an ague, or ipecacuanha a dysentery.

5. The relief of pain is always one of the earliest effects produced.

6. In acute cases, relief of pain and a fall of temperature generally occur simultaneously.

7. In subacute cases, the pain is sometimes decidedly relieved before the temperature begins to fall; this is especially the case when, as is frequently observed in those of nervous temperament, the pain is proportionally greater than the abnormal rise of temperature.

8. In chronic rheumatism, salicin sometimes does good where other remedies fail; but it also sometimes fails where others do good.

Regarding the action of salicin on the cardiac complications of rheumatic fever I have no experience. In Case 1, indeed, the muffled and indistinct character of the heart's sounds, which existed before its administration, disappeared with the general improvement which accompanied its use. But it needs not the details of cases to demonstrate that a remedy which curtails the duration, or mitigates the severity, of an attack of rheumatic fever, must of necessity diminish in a proportionate degree the risk of cardiac mischief. Neither is it doubtful that the general treatment most suited for rheumatic endo- or pericarditis is that which most surely and speedily cures the rheumatism. Rheumatic inflammation about the heart requires the same general treatment as rheumatic inflammation of a joint.

The dose of salicin is from 10 to 30 grains every two, three, or four hours, according to the severity of the case. Fifteen grains every three hours is a medium dose for an acute case. It is very possible that less might suffice; for I have not tried to find the minimum dose. It is very certain that a much larger dose may be given without producing discomfort.

Salicin is not soluble to any useful extent; it is best administered as a powder mixed with a little cold water. It is a very pleasant bitter. I have never found the least inconvenience follow its use.

When salicylic acid (originally prepared from salicin) was first introduced, I determined to try it; and in the one case in which I did have recourse to it, it seemed to do good to the rheumatism; but it caused so much irritation of the throat and stomach that I did not repeat it. This was, no doubt, due to its being impure; for Traube has lately been trying it in his

wards at Berlin, and reports most favourably as to its action in rheumatic fever.

It is the publication of these observations that has led me to give to the profession so soon my favourable and prior experience of salicin in the same disease.

I have no doubt that Traube's observations are correct, and that salicylic acid will be found efficacious in the treatment of acute rheumatism. But I have as little doubt that it is not so good as salicin for this purpose; for it is more apt to contain noxious impurities, it is not so pleasant to take, and it apparently requires a larger dose to produce its beneficial action.—*Lancet*, March 4 and 11, 1876, pp. 342, 384.

12.—ON SALICYLIC ACID.

By Dr. J. C. OGILVIE WILL, Assistant Surgeon to the Aberdeen Royal Infirmary.

Although salicylic acid has been much vaunted by continental surgeons, more especially by Thiersch and Kolbe, as superior as an antiseptic to carbolic acid, it has not, so far as I am aware, come into general use in this country, and the recent discussion at the Clinical Society will probably not do much to advance its cause. A pretty extended trial of it has, however, convinced me that in it we possess an agent of extreme value in a large class of cases; but I do not intend at present entering on a detailed account of the cases in which it is found useful, but simply to direct attention to its value as an antiseptic, to its advantages over other substances used for a like purpose, and to a few of the forms in which it may be employed, with brief notes of some cases as illustrative of its beneficial action.

Godeffroy, by a series of careful experiments, proved that salicylic acid is three times as powerful in its anti-fermentative action as carbolic acid. He used dough formed of yeast, flour, and water, to separate portions of which he added varying quantities of carbolic and salicylic acids, and noted the length of time that had elapsed before fermentation took place. In other experiments, milk, urine, beer, mustard-meal, and other substances have been employed, the results invariably proving that fermentation was either prevented or greatly retarded by the addition of salicylic acid. For example, fresh cow's milk mixed with 0·04 per cent. of salicylic acid, curdled thirty-six hours later than a similar quantity of milk to which no agent had been added; urine, to which salicylic acid had been added, remained clear and free from odour till the third day, while another specimen of the same urine, with which nothing had been mixed, was putrid. Its anti-fermentative

powers are already so well recognised by chemists in this country, that many of them add small quantities of the acid to such preparations as are liable to undergo fermentative decomposition, with most satisfactory results. These experiments are undoubtedly conclusive as to the value of the agent used as a preventive of fermentation, and experience has proved that it is equally reliable as a preventive and corrective of putrefaction when applied to sores and wounds, and that it materially assists reparative action.

In proof of its powers as a corrector of putrefaction, I may briefly refer to two cases: one of amputation of the thigh, at present under Professor Pirrie's care in the Aberdeen Royal Infirmary; the other, mammary cancer.

The former patient, a girl aged nineteen, was the subject of extensive necrosis of the femur; she was much reduced, and amputation was had recourse to, but with very grave doubts as to the ultimate result. The vessels were acupressed and water-dressing applied. As was expected, there was considerable suppuration, accompanied by great fetor. The applications used at this time were daily syringing with 1 to 40 solution of carbolic acid and dressings impregnated with red lotion. Salicylic ointment was then applied, and within two days the fetor entirely disappeared and healing action commenced. I on several occasions directed the attention of the students to the almost marvellous change that in this case followed the use of the new antiseptic. Another noteworthy point connected with the case was the extremely small, healthy, and highly vascular condition of the granulations with which the lips of the wound were studded, forming a marked contrast to those which Mr. Callender stated he had seen when salicylic dressings were employed. The ointment used was the one containing paraffin, the formula for which will be found below, and though it is a pretty strong one—1 part of acid to 7,—the patient repeatedly stated that it neither gave rise to pain nor smarting.

In a case of medullary cancer of the mamma, where the fungoid growth involved the entire gland, extended upwards nearly to the clavicle and well down into the axilla, and from which the fetor was so intense that the patient had to be removed to an out-of-the-way apartment, I ordered daily syringing with a 1 to 40 carbolic solution and a liniment of olive oil and salicylic acid, with which the sore was covered. By these applications putrefactive changes were arrested, the fetor disappeared, and the patient was rendered comparatively comfortable, while the attendants were not, as before, repelled from attending to her, which they had been by the disgusting odour pervading the atmosphere surrounding her.

These two cases of themselves show that salicylic acid is a trustworthy antiseptic. But carbolic acid has been proved beyond doubt to be so also, and boracic acid is largely used for similar purposes; therefore, unless salicylic acid possesses advantages over both, its substitution for either cannot be justified. It does, however, seem to me to possess certain advantages, which, though few in number, are not of less importance on that account; they may, I think, be best elicited by comparing carbolic and boracic acids with it. The two manifest objections to the use of carbolic acid, valuable agent though it be, are its irritating action and its smell. Salicylic acid can hardly be termed an irritant, and preparations containing only a small percentage of it can be thoroughly relied on as trustworthy antiputrescents, and it is odourless. These two points constitute its main claims to superiority to carbolic acid. The more important of the two is, however, not yet universally allowed, for, from the statements made by Mr. Callender and Sir W. Jenner at the Clinical Society's recent meeting, it would seem that in their experience salicylic acid had displayed most markedly irritant properties. But this is utterly at variance with the observations of continental observers, and also with my own, even where very much stronger preparations have been employed.

Preparations of Salicylic Acid.—Thiersch, who follows Lister's mode of treatment, with the exception of the substitution of salicylic for carbolic acid, employs cotton wadding or jute impregnated with the acid in place of gauze; but as the preparation of his dressings is a complicated process, and requires a lengthy description, I may refer to notices of his paper which appeared in a medical contemporary for May 26th and June 2nd of the present year, where full particulars are given. The watery solutions employed by him contain 1 part of acid in 300 parts of water. As an application to granulating surfaces a lotion containing 1 part of acid, 3 of sodium phosphate, and 50 of water has been found useful. As already mentioned, heat and the addition of borax increase its solubility; a clear lotion of considerable strength can thus be obtained: for instance, 10 grains of acid can be readily dissolved in one ounce of water by heating and then adding six grains of borax, or even 20 grains of the acid will be taken up by an ounce of water if 14 grains of borax are added. These solutions, in addition to their use as lotions, may be used as gargles, where an antiseptic and astringent gargle is indicated. To a surface where it can be accurately applied, and where the discharge is slight, though I have tried it with excellent effect where it was profuse, the cerate suggested by Professor Lister for rodent ulcer—salicylic taking the place of boracic acid—is a nice application. The

formula is from half a drachm to a drachm of salicylic acid, one drachm of white wax, two drachms of paraffin, and two drachms of almond oil; melt and rub up in a heated mortar. It should be spread on strips of muslin or fine linen.

Another ointment may be made of sperm oil, one drachm and a half; oil of theobroma, five drachms and a half; salicylic acid, from half a drachm to a drachm. This forms a thick paste, which should be thickly spread on lint. The heat of the surface acting on the oil of theobroma, a diffusible ointment is formed, which is a suitable application when it is desired to have the discharge thoroughly saturated with the antiseptic. An ointment less easily acted on by the body-heat consists of sperm oil and paraffin, of each a drachm and a half; oil of theobroma, two drachms; oil of almonds, one drachm; salicylic acid, from half a drachm to a drachm.

A very simple and most useful ointment, and one which answers admirably in some affections of the skin, is formed of half a drachm to a drachm of the acid to seven drachms of simple ointment. Though neither watery nor oily preparations of this acid belong to what is termed elegant pharmacy, yet a liniment of salicylic acid and olive oil will be found of much efficacy in burns; as an example, I may cite a case of burn, involving the upper arm and nearly the whole back of a child aged seven, where I recently employed it. Carron oil had been applied; and when my advice was requested, two days after, free suppuration had taken place, accompanied by great fetor. Lint soaked in olive oil and salicylic acid—12 drachms of the acid to 16 ounces of oil—was prescribed. The result was most satisfactory; the fetor disappearing, the suppuration rapidly decreasing, the pain greatly abating, and kindly healing action taking place. In less than a fortnight the whole surface was healed, with the exception of two small spots covered by healthy granulations, to which red lotion was applied. I have since found that a much weaker oily solution (two drachms of the acid to eight ounces of oil) was all-sufficient in a case of severe scald of the foot lately under treatment.

For cancerous sores Thiersch recommends dusting with the pure acid, or with equal parts of the powder and starch, or powder formed of charcoal and the acid might be employed for the same purpose, or for dusting over the face of poultices applied to sloughing surfaces. Endless preparations might be devised; but those I have described seem to me as useful as any combinations with which I am acquainted, and therefore I shall leave others to modify them in any way that may suit their tastes or modes of treatment.

Though I stated in the outset that I did not intend entering on details of cases, yet I cannot refrain from urging its use in

eczematous affections, for nowhere is its beneficial action more marked than in them. In the eczema of children, more especially in that found affecting the head and face, it is far superior to any remedy I have yet employed; it was, in fact, the results attending the use of salicylic acid in a case of the kind that first convinced me of its value, and I have since found that others have formed a like estimate of it in similar cases. A child, aged 18 months, had been under my care for many weeks with a patch of eczema rubrum on the cheek; I had tried all kinds of local applications, and had given arsenic and iron wine internally, but with no avail; for though at times it seemed to be giving way to treatment, a fit of itching, I suppose, had caused the child to rub it, and increased irritation always followed. I had not before used salicylic acid, and had but little faith in good accruing from its exhibition, but as a last resource I prescribed an ointment containing one drachm of acid to an ounce of lard. When I next visited the child, a few days after, the patch had nearly healed, and very shortly after it was perfectly whole. In another case the result was almost equally striking, but here it was the first application used. A child, aged six months, had a patch of *E. impetiginodes* on the occiput; it had first appeared about a month before. As some parts were thickly covered by crusts, a poultice was used to detach them, and then salicylic ointment was rubbed in. In six days every vestige of disease had disappeared. A brother and sister, aged three and five, were the subjects of eczema narium of short standing. Salicylic ointment was applied on a Friday, and on the following Monday the surfaces were perfectly healed. It is unnecessary to occupy space by entering on details of other cases, but I may simply say that in very many I have found it answer admirably.—*Lancet*, Dec. 18, 1875, p. 870.

13.—ON DIPHTHERIA.

By Sir JOHN ROSE CORMACK, K.B., F.R.S.E., Physician
to the Hertford British Hospital of Paris.

Diphtheria is a specific, contagious, asthenic, general disease. It sometimes occurs as an epidemic; but is always present in a sporadic form in some districts of country and in some towns—in Paris, for example.

The leading characteristic of diphtheria is an exudation, on mucous surfaces, wounds, and cutaneous abrasions, of caco-plastic lymph, which becomes the basis of a tough, leathery, coherent false membrane, which may be met with on the mucous membrane of the pharynx, larynx, trachea, bronchial tubes, nares, eyelids, and vagina, as well as around wounds.

This false membrane often forms on the edges of the tracheotomist's incision in cases of laryngo-tracheal diphtheria.

The pellicle of diphtheria is a leathery membrane, tough and difficult to tear. It is not the product of "inflammation" in the ordinary acceptation of that term. It has as its basis a peculiar fluid exuded from the surface on which it rests. This fluid, which may be described as cacoplastic lymph, agglutinates a profusion of epithelial cells. The membrane thus formed consists of compact tenacious layers, which give it a stratified appearance.

The pellicle of scarlatinous sore-throat, on the other hand, is pultaceous and easily torn. In cases of scarlatinous gangrene of the tonsils, the substance which simulates the false membrane of diphtheria will, on examination, be found to be spha-celated mucous membrane, with the addition of a pultaceous substance—a loose epithelial detritus. In a word, the important diagnostic distinction consists in the diphtheritic pellicle being a strong, tough membrane, and the scarlatinous simulation of it being a pultaceous, easily-torn stratum of detritus, or a portion of gangrenous mucous membrane.

The formation of the diphtheritic pellicle generally begins on one or both tonsils after two or three days of some precursory redness and swelling, which are often so slight as to be unobserved. The pellicle, in most cases, first shows itself as a white patch of exudation on one tonsil: from this starting-point, formation of false membrane proceeds—generally, but not always, in continuity—rapidly to both tonsils, the veil of the palate, the uvula, and the pharynx. This extension is accomplished in young children in from one to three or four days, and in adults less rapidly. From the pharynx, except in mild cases, the false membrane descends into the larynx. It is not so in scarlatina, the larynx being comparatively seldom affected in that disease. To quote the familiar clinical saying of Trousseau—"Scarlatina has no liking for the larynx."

Nevertheless, there exists in some cases a great difficulty in establishing an absolute diagnosis between diphtheria and scarlatina, in consequence of the unquestionable relationship which exists between the two diseases, notwithstanding their being pathologically distinct from each other. Diphtheria—as Graves and others have shown, and as Trousseau admitted in his later teaching—often follows so close in the wake of scarlatina as to seem to be one of its later stages. False membranes—diphtheritic membranes—form in the pharynx and larynx, as an immediate sequel of scarlatina. In such cases, diphtheria has closely followed or been engrafted on the original attack of scarlatina.

Discussions — unprofitable, meaningless discussions — have taken place in respect to the identity or non-identity of *diphtheria* and *croup*.

Diphtheria is the disease *sui generis* which has now been briefly described. Croup is not a disease—it is a name which authors sometimes apply to laryngo-tracheal diphtheria, and sometimes to several other diseases, which are essentially different in their pathology from diphtheria and from one another. If the word “croup” is to be retained in medical literature, it must, to avoid confusion, be applied to only a symptom—that is, to *stridulous breathing*, a symptom common to laryngismus stridulus, stridulous laryngitis, laryngo-tracheal diphtheria, and some other affections.

Most French physicians of the present day, adopting the views as well as the nomenclature of Bretonneau and Trousseau, apply the names *croup* and *vraie croup* to the membranous sore-throat which is the local manifestation of the general disease first called by Bretonneau, and now usually designated, *diphtheria*. They apply to laryngo-tracheal diphtheria, the terms *croup* and *vraie croup* (true croup), to distinguish it from the *faux croup* (false croup), the non-membranous affection in which spasm with stridulous breathing is the predominating feature. Many of them in this way include together the non-inflammatory spasmodic affection (laryngismus stridulus) with laryngitis, which is nearly always a stridulous affection in infants. It is the “inflammatory croup” of various English authors. Francis Home, of Edinburgh, represented it as a stage of membranous croup (diphtheria). In his famous little monograph, he only describes eight cases, and of these, five are tracheal diphtheria and three laryngitis. As no false membrane was seen in these cases, he—like many of his successors—jumped to the conclusion that the false membrane was absorbed, or its formation prevented by the treatment. For a long period Home’s successors, British and foreign, continued to make the same mistake—and, indeed, till Bretonneau published the result of his clinical study, the error now adverted to was universally accepted as the truth. Cheyne mixed up tracheal diphtheria with laryngitis; and attributed the recoveries from the latter to the energy of the bleeding and purging—and the deaths from the former to the omission or imperfect adoption of that treatment. In 1810, the Emperor Napoleon offered a prize for the best treatise on “croup.” All the competitors confused together laryngismus stridulus, common inflammatory laryngitis, and laryngo-tracheal diphtheria. Most English authors, when they write about “croup,” really mean laryngitis and laryngo-tracheitis. If the term “croup” be so interpreted, it is of course totally

different from "diphtheria," the disease which is the subject of this paper.—*Edin. Med. Journal*, March 1876, p. 799.

14.—DIPHTHERIA.

By Dr. ROBERT BELL, F.F.P.S.G., &c., Glasgow.

Perhaps the most difficult problem in medical science which remains to be solved is that which relates to the causation of zymotic disease. All are certainly agreed that each infectious disease is due to a specific poison which, by one way or another, gains access into the body of its victim; but what the nature of the entity is, and in what manner it enters and attacks the human subject, is still a mystery. I say, in what manner it *enters* the system of the patient is still not understood. I feel convinced that too much has been taken for granted, and accepted as fact on this point; and I am certain it will not be until this question has been satisfactorily settled that we shall arrive at correct conclusions as to the nature of the different poisons which we call contagia. It is all very well for us to assert that the disease-producing essence is inhaled by the breath, and thus gains access into, and produces its baneful effects upon, the individual. I hold that we have no proof of this theory being correct. It was at one time thought that typhoid fever was infectious; that it was by inhaling the poison in the process of breathing the disorder was contracted. I feel that those who have had most to do with the treatment of this malady will agree with me that typhoid is not infectious in this sense. Other instances might be cited, showing that our opinions on this important subject have recently undergone considerable change. In diphtheria especially, it appears to me that our conceptions with regard to the mode of ingress of the poison are very far from correct, and will not yield the fruit which we much desire to reap; viz., an effectual means of destroying the disease, and thus saving the lives of our patients. Some years ago, it struck me that, when diphtheria attacked a patient, the *modus operandi* was the following. The germs of the disease become located on a surface which provides a favourable soil for their development and multiplication, just in the same way as the germs of typhoid select the mucous membrane of the bowels. In the disease under discussion, the locality chosen by the poisonous particles is the throat and the neighbouring mucous surfaces. Here these *materies morbi* implant themselves, becoming attached by the tenacious and viscid secretion of the tonsils, the warmth and moisture of the part favouring their further development and progress. A dense fungoid growth is the result, at first of limited extent, but gradually encroaching upon the surrounding healthy mucous

membrane. The very presence of this deposit—I refrain from calling it an exudation—results in inflammation of the subjacent and surrounding tissue. We may, and often have, a diphtheritic deposit without the slightest constitutional disturbance. I have often seen diphtheria in its early stage without the general system having apparently been affected in the slightest degree; and I venture to say that diphtheria, in its incipient stage, rarely affects the general health. Moreover, if the patient be strong and robust, some time will elapse before constitutional symptoms will manifest themselves. On the other hand, if the victim be weakly and in feeble health, or if his vital energies have been laid low by breathing foul gases, the disease will run a rapid, and in general a fatal course. From what has been said, it will be perceived that I conclude diphtheria to be, in its first stage, purely a local disease, exactly as a chancre, at its commencement, is syphilis in the part only, not having yet affected the general system; or, to take another example, just as vaccinia, in its primary stage, is purely a local lesion. Another example may be cited; viz., the snakebite, which, if caught in time may have its venom limited to the part bitten. It is, therefore, in this stage of the disease that an effectual and speedy cure can be guaranteed. When, however, the disease has for some time established itself on the tonsils, poisonous matter from the film becomes absorbed, first by the lymphatics, as indicated by the hardening and enlarging of the neighbouring glands, and then the general system becomes impregnated, and it is at this time that the greatest danger threatens the patient; the vitality becomes reduced, and the poisonous film spreads with increased rapidity, the poison becoming multiplied with most deadly speed within the body. The above conclusions have been gradually arrived at after carefully observing a large number of cases of this dreadful disease. It is now time to say a word or two on the nature of the deposit, which is the principal feature of diphtheria. This always begins in one or more minute specks or points. These gradually enlarge to such an extent, and coalesce so, as sometimes to cover the whole area of the throat, and often the palate, posterior nares, and larynx. On its first appearance, it most closely resembles an aphthous spot, and indeed the aphthous and diphtheritic diseases bear a very close resemblance to each other in many ways. They both attack the mucous membrane of the mouth and throat; their appearance to the eye is similar; they both indicate a weakened condition of the general health; they are both fungoid in their nature; and I am not sure whether the one may not degenerate or merge into the other. Bearing these points in mind, it is always a safe plan of treatment to destroy in the early stages any deposit of a suspicious

appearance on the tonsils or other surface of the throat; and it now remains to be seen how this can be accomplished. If what has been said anent the nature of the film, and the portal by which the disease enters the circulation, be correct, it follows that, if we can destroy the poisonous quality of the film while the disease is yet local, we keep it in that condition, and prevent its further effect on the health of the patient.

I may premise my remarks on treatment by stating that, since I have adopted my present method, I have only lost two cases from this disease; and these were children who resisted me to such an extent, that it was absolutely impossible to employ the treatment at all. It has been my lot to treat a large number of patients suffering from this disease, and it is with gratitude that I say with almost uniform success. My first consideration is to view the disease as one entailing rapid and severe prostration. This thought impels me to insist on free stimulation and plenty of nourishment, in the shape of soups, jellies, and milk, and this from the very onset of the disease, so as to assist the *vis medicatrix nature* to combat successfully the disease, and, if possible, expel it from the system. The grand aim is to endeavour to prevent the vital energies from succumbing to the fearfully prostrating effect of the poison. This dietetic treatment must be simultaneous with local and general medical treatment; but the most important, in my opinion, is the local application of substances which destroy the poisonous properties of the deposit on the throat. This consists of carbolic and sulphurous acids along with the liquor ferri perchloridi. My application generally consists of carbolic acid, one part; sulphurous acid three parts; solution of perchloride of iron and glycerine, of each four parts. This is either applied with a large camel-hair pencil, or by means of the spray-apparatus, at intervals of two hours. The mouth should also be frequently rinsed out with a weak solution of Condyl's fluid in water, and the following mixture taken in dessert-spoonful doses every two hours:—*R.* Potassæ chloratis ʒ iij; acidi sulphurosi ʒ iijss; tincturæ ferri perchloridi ʒ iij; glycerini ʒ i; aquæ q. s. ad ʒ vi. *M.* In this way, a medicament is applied to the throat every hour, and, to be successful in curing the disease, this energetic treatment is absolutely necessary. Of course, if the patient be sleeping, the usual rule must be observed; viz., never disturb a patient if asleep.

In conclusion, I may remark that the presence of albumen in the urine must not be looked upon as a necessary symptom of diphtheria, as it often does not manifest itself till far on in the disease; and, on the other hand, I have often observed albuminuria as a concomitant of ordinary sore-throat. It

should also be remembered that, at certain periods of the day during the progress of digestion, albumen can often be detected in the urine when no disease is present.—*British Medical Journal*, Jan. 29, 1876, p. 131.

15.—THE UNIVERSAL DISINFECTING POWDER.

As is well known to most, disinfectants belong to two groups—viz., such as attack and destroy bodies already in a state of decomposition, or about to pass into such a state; and those which hinder bodies from becoming decomposed. To the former group belong such substances as chloride of lime and permanganate of potash; of the latter, carbolic acid is the best exemplar. Moreover, as some of the most noxious and disagreeable products of decomposing matter are either gaseous or suspended in the air, a proper disinfectant should be capable of attacking aëriform bodies—i.e., should be itself in the gaseous state, in part at least. There is only one element which combines all these properties, namely, chlorine. This has the power of destroying, or combining with, nearly all putrescent compounds; whilst the chlorides themselves have notable powers of arresting decomposition. Even the simplest and best known of these—common salt—is useful in this way. In no form have we seen these properties so well utilised as in the Universal Disinfecting Powder, which unites the double properties of containing preserving chlorides, and of giving off, when wetted, nascent chlorine; thus exercising a twofold power of attacking putrescent and putrescible matter. The substance itself somewhat resembles bay-salt, and gives off a weak odour, far from unpleasant, of chlorine gas. It may be used either in the solid form or dissolved in water, and, being composed of alkaline or earthy chlorides, when so dissolved does not possess the acrid properties of a simple solution of chloride of zinc, and consequently is not poisonous like the well-known Burnett's fluid. In either form, after due trial we can amply recommend this new disinfectant as likely to be a boon to the public—efficient, yet safe. Might we suggest, however, to the proprietors the usefulness of that system, now adopted for so many things, of having the lid perforated, should this be compatible with their object in furnishing an efficient disinfectant?—*Medical Times and Gazette*, March 25, p. 347.

This new disinfecting powder meets a great public want. It is cheap, highly effective, and free from the danger of producing accidental poisoning to which other well-known disinfectants give rise with such lamentable frequency. It is perfectly

soluble, colourless, and free from odour. These are qualities to make it valuable for daily and popular use. It is manufactured by Messrs. Ledger and Co., Lant Street, Southwark. It has been found very effective by many medical officers of health who have practically tested it, and has been favourably reported on by Mr. Wanklyn and Mr. Bartlett, two of our most eminent experts in the chemistry of disinfection, and will now no doubt find its way into popular use in towns and rural districts, and in ships, hospitals, slaughter-houses, stables, &c.—*London Medical Record, March, p. 143.*

16.—ON CHLORIDE OF LEAD AS A DEODORISER AND DISINFECTANT.

By Dr. R. H. GOOLDEN, London.

I wish to call the attention of medical practitioners and especially of sanitary officers, through the *Lancet*, to the value of chloride of lead as the most powerful and economical agent for eliminating sulphide of hydrogen from the atmosphere, as well as from all organic matter in a state of decomposition or putridity. There is nothing new in this fact. It is known to all chemists, but its application has been practically disregarded, owing to the popularity of other deodorisers, which having been made the subjects of patents are well advertised, and therefore generally adopted, and they certainly do answer the purpose with more or less completeness. Of these I may instance—Chloride of zinc (Burnett's), chloride of aluminium, hypochlorite of soda (Marvel Fluid), hypochlorate of potassa, hypochlorate of lime, permanganate of potassa (Condy's), permanganate of lime, peroxide of iron, peroxide of manganese, powdered charcoal, bog earth, and alluvial earth.

I need not enter into the *rationale* of the chemical action of these several ingredients, nor do I suggest the substitution of chloride of lead for the three last-named substances when it is proposed to convert organic matter into reproductive soil for farming or gardening purposes, but only in those cases where it is necessary to purify a fetid atmosphere, which has to be breathed by living people.

It has already been objected that zinc and manganese have been detected in plants grown in soil manured by stable-dung, which had been deodorised by Burnett's and Condy's fluids. I do not know how far the fact has been established, but even the suggestion is of such importance as to make us very careful in proposing the use of lead, unless it can be shown that lead is not in the same category. I think we may arrive at a strong inductive probability that it is not so; for the lead sulphide is quite insoluble except in strong nitric acid, in boiling hydro-

chloric acid, and in aqua regia (which is not the case with zinc or manganese); nor is lead sulphide absorbed in the human body when swallowed, but passes on by the bowel as harmless as powdered charcoal; and the affinity of lead for sulphur is so strong that lead sulphide cannot be resolved but at a very high temperature. Another safeguard lies in the very sparing solubility of lead chloride, and also in the very small quantity used in the deodorising process.

The chloride of lead and sulphide of hydrogen react with a simultaneous double affinity, producing an insoluble lead sulphide and hydrochloric acid. Lead is so readily acted upon by sulphur, that metallic lead and oxide of lead in paint, even when protected by the dried oil, is turned black in any atmosphere where sulphide of hydrogen exists. White paint in stables, waterclosets, and in artists' paintings, is soon turned black by it and spoilt.

To prepare it for use, take, for ordinary purposes, half a drachm of nitrate of lead, dissolve it in a pint or more of boiling water—(nitrate of lead is a soluble salt, and very cheap; it may be had in any quantity for about a shilling a pound, and should be much cheaper if bought in large quantities),—and dissolve two drachms of common salt (chloride of sodium) in a pail or bucket of water; pour the two solutions together, and allow the sediment to subside. The clear supernatant fluid will be a saturated solution of chloride of lead. A cloth dipped in this solution and hung up in a room will sweeten a fetid atmosphere instantaneously, or the solution thrown down a sink, water-closet, or drain, or over a heap of dung or other refuse, will produce a like result. Even the tarnishing of gold and silver plate may be prevented by a rag dipped in the solution being hung up in the room or window where it is exposed.

It will thus be seen that one great advantage of chloride of lead over all other deodorisers lies in its great cheapness. The cost of the quantity of nitrate of lead required for use may be estimated at a fraction of a farthing; it is easily carried about, or may be sent by post; it is a dry solid, is not caustic, nor will it discolour or blister the skin; it can be kept in paper or a pill box, and therefore does not require a glass or other fragile bottle. The exact equivalent proportion of nitrate of lead and chloride of sodium would be 100 to 317 in weight. As it is necessary that the whole of the nitrate of lead should be converted into chloride, a slight excess of salt is desirable, but as chloride of sodium in the solution renders the chloride of lead even less soluble than in water, I have adopted the weights respectively as half a drachm to two drachms—sufficiently correct for all practical purposes. 331 grains of nitrate of lead

converted into lead chloride are sufficient for 34 grains of sulphide of hydrogen. Now 36·38 grains of sulphide of hydrogen represents 100 cubic inches—a quantity quite ten times larger than under ordinary circumstances we are likely to have to deal with.

In sick rooms, hospitals, and crowded assemblies, we have to deal with the sulphide of hydrogen and ammonium, given off by the resolution of organic, especially albuminous matter in decay, or eliminated from the living body in fæces through the skin and with the breath; and it must be borne in mind that whenever great numbers of people are crowded together in rooms, as in balls, concerts, theatres, or schoolrooms, the human body gives off an amount of sulphide of hydrogen, and that it is this, and not the carbonic acid, that makes people exposed to such an atmosphere so depressed, and which, when highly concentrated, develops typhus poison. The quantity is indeed very slight compared with its power of offensiveness, as in the analogous case of various flowers whose scent is most powerful, producing a sensible effect upon the human organism, and yet the agreeable or offensive matter composing it is too small to be detected by any test except the olfactory sense; and hence it is that the small quantity of lead chloride contained in the solution is found practically to be quite sufficient to sweeten the most offensive room, and, in my experience, to deodorise any drain, water-closet, or sink.

A not less important direction, in which the use of the chloride of lead would prove of the utmost value to H.M. navy and the mercantile marine, is for the purification of bilge water, and of the close, fetid atmosphere between decks in emigrant and passenger ships; easy of transport, occupying little room, and safely stowed away in paper or wooden boxes. The sea-water requiring no addition of salt, the solution is made without trouble, and need merely be thrown down into the bilge, where the ship's motion completes the process.

The following cases, which have occurred in my own practice, will illustrate its use and successful application:—

Case 1.—A lady living in Chester-square, who was suffering from sciatica, consulted me many years ago, and my attention was called to a large tumour in the external part of the hip-joint. The tumour was soft and not very prominent, had been very gradually increasing, and with its increase the sciatic pains became more severe. Believing it to be a fatty tumour pressing on the sciatic nerve, I could only refer her to a surgeon for its removal. It was accordingly removed by Mr. Lane, and without any hemorrhage at the time, and proved to be a large fatty tumour the size of a child's head. The sciatic pains ceased, and the patient appeared to be in a fair way to

recover speedily the effect of the operation. About a week afterwards I was asked to meet Mr. Lane, and found that the cavity from which the tumour had been removed had been distended with blood in a state of decomposition, the patient suffering all the symptoms of blood-poison: small rapid pulse, black sordes coating the mouth, tongue, lips, and nose; wandering, muttering delirium; she would take no food. The room was most offensive, the smell penetrating to the passages and other rooms. Basins containing Condry's fluid, Burnett's fluid, and bog-earth were in various parts of the room, but exercised little influence over the fetid emanation.

Wishing to try the effect of the lead chloride, we obtained, with great difficulty, a small quantity of nitrate of lead, and prepared the solution as above described. A towel dipped in it was hung up in the room, and *instantaneously* the smell disappeared. The wound was dressed, and over the wound some lint dipped in the solution was placed. The next day we met, and found our patient sitting up in bed quite cheerful, with a clean tongue and good pulse, enjoying a mutton-chop and some port wine. The room was perfectly free from smell.

I have had many other opportunities of using this solution with equally satisfactory results, especially in paralytic cases, where, towards the end, the fetor of the breath and emanations from the skin are most offensive.

Case 2.—Three years ago, after a very severe storm-shower, the large drain at the back of the University Club was burst by the flood, and opened to the atmosphere. A more distressing smell in its immediate neighbourhood could hardly be imagined. Carbolic acid was used in the Club, which substituted only one stink for another. I sent the steward for a pound of nitrate of lead, of which half an ounce was used. A towel wetted with the solution was hung up in each room, and the rest poured down the drains and into the sewer, and immediately all smell was gone. The neighbours, who were unaware of what was being done, expressed the greatest surprise at the sudden disappearance of the nuisance. Although the drain remained open for some days until it could be repaired, there was no more inconvenience felt from the smell.

Case 3.—The wife of a horse-jobber in a very large way of business, apparently suffering from the poison of carbolic acid used to disinfect the stables where she lived, consulted me. She informed me that although the stables were well-drained and well-ventilated, the effluvium from the large number of horses and the accumulation of dung was so offensive that they had been induced to use carbolic acid, which, though painfully disagreeable, was not so offensive as the stable exhalations had been. It appeared also that since the use of carbolic acid they

had lost seventeen horses, and that they could not help connecting their loss in some way with it. I recommended the immediate disuse of the carbolic acid, and in its place to try the nitrate of lead and salt. In a fortnight I saw her again in perfect health, and learnt that the stables had, from the first application, become freed from all disagreeable odour, and that the dung-pits into which some of the solution was regularly sprinkled were now sweet, to the great contentment of the horsekeepers and stablemen, with whom the removal of the dung has always been a subject of great difficulty.

Case 4.—Some years ago, when visiting one of H.M. largest ships, I casually heard loud complaints of the nuisance caused by the bilge water. Through the courtesy of the officers, which was greater than their faith in any possible remedy, I was allowed to try the experiment of removing it, and, having procured half an ounce of nitrate of lead, it was dissolved in a bucket of boiling fresh water, and thrown down the bilge when the ship was rolling slightly. The effect was the instantaneous disappearance of all smell; a large white precipitate, which immediately afterwards became black, subsided to the bottom, and the bilge-water became perfectly clear. The cost was one halfpenny.

I have selected these few cases as leading types of the application and results of chloride of lead as a deodoriser and disinfectant, and only trust that they may be sufficient to lead my readers to continue the experiments in different directions, as I have hitherto found it to be the most simple, the most economical, and most successful of all the processes which have come under my notice.—*Lancet*, Dec. 11, 1875, p. 828.

17.—ON THE SUBCUTANEOUS INJECTION OF QUININE.

By Surgeon-Major GEORGE YEATES HUNTER, Presidency Surgeon, Bombay.

[The subcutaneous injection of quinine in malarious fevers has been largely practised in India, especially when rapid action is desirable, or when from irritability of the stomach the drug is not well borne in the usual way of administration.]

I have often tried it in scattered cases, and not unfrequently been disappointed in its action, and embarrassed by its sequelæ in the shape of troublesome ulcers. However, thinking the latter might have been due to want of sufficient care, or to the employment of a solution too acrid for the purpose, and knowing that in some obstinate cases I had found fever which had baffled me when adopting the ordinary treatment, yield to this mode of administering the great specific remedy for malarious fever, I determined to give subcutaneous injection of quinine

a fair trial in the Bombay House of Correction, using six minims for a dose of the following preparation:—Citric acid, three hundred grains; quinine, eighty grains; distilled water, one ounce. To be dissolved with the aid of heat (six minims of this solution being equal to one grain of quinine).

I think it may be conceded that the fever yielded as soon as when treated per os, if not sooner, the table showing that in most cases the attack was arrested on the second day, and in no case later than the fourth, notwithstanding the irregular mode of treatment as to time &c. consequent on the patients being prisoners in a gaol.

Beyond all question there was a great saving in the quantity used, the cases being conducted to a satisfactory conclusion, so far as fever was concerned, with a very moderate expenditure of quinine, say three grains or so. On the double ground of checking the fever and economy in the consumption of the drug, this mode of treatment would seem to be superior to the old method, but, unfortunately, a drawback presents itself in inflammation at the point of insertion of the syringe. And, of the cases treated, two only escaped, all the rest having been more or less affected, some of the inflamed punctures assuming the form of inflamed vesicles, and others that of unhealthy ulcers, and in every case there was pain enough to disable the patient from working with that hand, the ulcers taking from six to seven days to heal.

The two who enjoyed immunity were healthy subjects of sound constitution, which would lead to the inference that the cases for this treatment ought to be selected.

I need scarcely say that most of the prisoners are men of dissipated habits—cachæmic, cachectic, and many completely broken down by a career of debauchery, a large proportion being opium-eaters or ganjah-smokers, or addicted to excessive use of country liquors—men constitutionally so unsound that the slightest abrasion may cause after-trouble.

I think I am justified in drawing the following conclusions from my experiments on this series of cases:—

1. However well adapted subcutaneous injection may be to selected cases—well fed and set up, and of naturally good constitution,—it is inadmissible in the unhealthy class who fill our gaols.

2. The method is not really economical, on account of the loss of labour and consequent diminished outturn.

3. Time is not gained, as, although the cure of fever was speedily effected, a week was lost subsequently if ulceration occurred.

4. The operation, slight though it be, is by no means painless, however carefully it be performed. And it is not

unattended with risk. I have heard of one fatal case of tetanus which followed its use, and appeared to be caused by it.

It seems to me that I have advanced sufficient arguments to throw a doubt upon the advisability of hypodermic injection of quinine in the cases of convicts. Those who gain a livelihood by manual labour would find the sequel inconvenient should it occur, which, however, is less likely than in the case of broken-down prisoners; and in the army, where its value has been tested on a large scale, it has been found that men have been kept from drill on account of the subsequent suffering, even if the inflammation set up around the puncture did not run into ulceration.

While the result of my experiment would not invalidate the argument in favour of hypodermic injection in suitable subjects, still it would seem to establish the advisability of carefully selecting the cases. I certainly would not venture to adopt it in civilised private practice, although it might be expedient in the cases of villagers down by the dozen with fever, which holds good in some malarious hotbeds up country in India, more especially in severe and intractable forms of fever, when rapidity of action might save life, and when economy in expenditure becomes an object on account of the number treated.

Surely it is rather a barbarous procedure to perform a surgical operation, however slight it may apparently seem, to attain a medical object, when such object is attainable by other means.

My impression is that when the orthodox channel is closed in consequence of the intolerance of the stomach, or otherwise, and when the weakly state of the patient renders it not unlikely that the subcutaneous tissue may resent this short cut into the system at its expense, the wiser plan would be to exhibit the drug per anum; but the circuitous route per os, when practicable, is incomparably better and safer, and quicker too, should the short cut have the penalty of ulceration attached to it.

On the recommendation of an officer of the Sir Jamsetjee Jeejeebhoy Hospital, I have since tried the solution of the neutral sulphate with great success, so far as checking the fever was concerned, and with certainly less trouble from sequelæ; yet, although more cases escaped ulceration, whilst I write the worst case I have seen is under treatment, and which followed the use of the neutral solution two months ago. So, although it is a better preparation than the other, barring the crystallisation of the salt and necessity of using hot water to obviate the clogging of the syringe, which, moreover, requires constant washing and oiling, and quickly wears out, still I am

not prepared to say that even this solution should be of general application.—*Lancet*, April 8, 1876, p. 527.

DISEASES OF THE NERVOUS SYSTEM.

18.—ON THE VALUE OF ELECTRICITY AS A THERAPEUTIC AGENT FOR THE RELIEF OF PAIN.

By Dr. LESLIE JONES, late Surgeon to the West Cornwall Hospital, &c.

[In the following paper those cases only are referred to in which pain constitutes the chief subjective symptom.]

I am convinced that comparatively few physicians give electricity that credit which is its due, and thus this most useful agent, especially in respect to the relief of pain, is too generally allowed to remain neglected.

The following cases fairly represent what can practically be done with electricity for the relief of pain; they are chosen from a considerable number, but any of the others would equally well answer the object I have in view; and this will be accomplished if any one who has not already tested the value of electricity will be induced by looking over these notes to give it an impartial trial.

Stöhrer's continuous current battery with thirty cells is the one I use, and I have not yet met a case in which a greater number of cells was required: it may be well to add that the sponges supplied with the machine are far too small. I use whole Turkey sponges, from two to three inches in diameter, firmly tied over the ends of the ordinary electrodes; by this means, the sponges being well soaked in water, or salt and water, the burning sensation so much complained of, especially when the application is made to the rectum, is greatly reduced. In every case it is better to begin the application from a few cells only, and gradually increase the strength of the stream to the point required, as the greatest difference in the effects produced will be noticed in different individuals. In cases too where the sponges have to be applied to the head or spine the patient should constantly be asked if there is any dizziness, and if this supervenes the application should be immediately discontinued.

Facial Neuralgia.—Two forms of facial neuralgia are described by Dr. Althaus; "a mild one and a severe one, the former comes on after exposure to damp and cold, or after mental emotions, or is owing to caries of a tooth; it is not made worse by moving the face, and it occurs at all periods of life; while the latter occurs generally without any apparent cause, is almost entirely confined to advanced age, and is

brought on or made worse by the least movement of the face." I shall illustrate the effects of galvanism in two cases of the first form of the disease, and in one of the second form, or Fothergill's disease; in which operative treatment frequently, and medicinal treatment invariably proves useless. In this instance (Case 3) the beneficial effect of galvanism was most striking.

Case 1.—Tic-douloureux of two years' standing relieved by one application of galvanism.—Miss N., æt. 24, came under my care in June last, suffering from neuralgia of the left side of the face and which, without cessation for three days previously, had caused intense pain; during that time she had not taken solid food, had not slept, and now appeared to be fairly worn out with pain and want of food and sleep; otherwise her general health was excellent and the teeth were perfect. Two years ago the neuralgia had come on after a debilitating and protracted illness, and since then had tormented her with variable intermissions, seldom, however, leaving her for long. She had got so tired of trying different remedies that, to use her own words, she "believed the only thing to do was to let it wear itself out." At the present moment her face was blistered from some liniment she had been using; it is difficult to describe the agony she was apparently enduring, walking about the room imploring me to do something to ease the pain. Using six cells of the continuous-current battery, I applied the moistened sponge of the negative pole in front of the ear and on the temple, and the second sponge interruptedly over the infra-orbital region and on the cheek. I had scarcely applied the sponges when she seized the handles of the reaphores and tightly pressed the sponges to her face declaring that the pain was going. I continued the application for five minutes, when she said the pain had gone. The day after this the lady came to me stating that she had slept well and enjoyed her food and had had no return of the pain, but asked for another application in case she should. I have just now heard from her that, since that time, now six months ago, she had been perfectly free from pain.

Case 2.—Neuralgia of the face due to pregnancy cured by three applications of continuous current.—Mrs. B., æt. 37, five months advanced in her first pregnancy, complained of great and unremitting pain in the left side of the upper jaw; two teeth had been extracted, different applications used, quinine and various medicines taken, but all with the same result, the pain continued. I applied one sponge, the negative pole, over the infra-orbital foramen, the positive sponge inside the mouth, over the painful teeth, gums, and antrum, retaining the negative sponge in position and slowly moving the

positive sponge over the painful parts. I used the current from six cells, and continued the application for five minutes. The lady left my house completely relieved, but returned next day, the pain having recurred. I repeated the galvanism twice, and during the remaining three weeks of her stay in Blackpool there was no return of the pain.

The beneficial effect of galvanism in this case was especially interesting, as the neuralgia was no doubt due to the existence of pregnancy, and these cases are noted for the persistent character of the pain even when the painful teeth have been extracted.

Case 3.—Fothergill's disease; pain relieved after three applications of galvanism.—I was asked to visit Mrs. —, an old lady of nearly 80 years, whom I found in bed in a most prostrate and exhausted condition. For years past she had suffered intense paroxysms of pain in the right brow, temple, and down the right side of the nose; but she described the present attack as the worst she had ever endured. The eye could scarcely be seen owing to the puffed and swollen state of the forehead, eyelid, and nose; this she described as erysipelas, with which, she said, the painful attacks invariably terminated; in fact, she looked upon the erysipelas and the pain as cause and effect, the latter only abating when the former "came out." I tried to assure her that there was no erysipelas, that the tender and swollen condition of the face was owing to neuralgia, and that I believed nothing would give her permanent relief except the application of electricity; but in this I failed, and was forced to content myself with prescribing pills containing quinine, belladonna, and cannabis indica. As might be expected, without effect, the pain continued. At last, after much persuasion, she was induced to let me try the electricity. I began with only two cells of the continuous battery, and used very gentle pressure, because I was not certain that I had not to deal with a case of neuritis of the trifacial nerve, but the application of galvanism quickly set the question at rest; for independently of there being no anæsthesia or muscular palsy, the patient herself requested that I should "press harder" and increase the power of the current. Dr. Althaus lays stress on this fact as a reliable means of diagnosis between neuritis and the severe form of neuralgia; in the former disease patients dislike the application of the continuous current, in the latter they relish it. Increasing the strength from four to six cells, I applied the sponges irrespective of the poles, at short intervals all over the painful parts and continued the application for five minutes. Considerable relief was afforded by the first application.

On the following day the swelling was scarcely visible,

though the surface of the face was still very sensitive; after the third application the pain had gone. For some weeks after, at the old lady's request, I applied the galvanism every third or fourth day, using two or four cells; her health greatly improved, her appetite returned, she was able to take long walks, and left Blackpool at the end of two months perfectly well.

Sciatica.—I have treated so many cases of sciatica with the continuous current, and so invariably with the same result, that it is quite at random that I choose the two following cases; the effect of the current when thirty cells are used is almost certain; the pain ceases and temporary numbness of the limb is produced. I believe that when applied in the manner described in Case 6 the conductivity of the nerve is partially arrested.

Case 6.—Mr. J., æt. 45, sent for me five months ago in consequence of a severe attack of sciatica in the right thigh. I found him sitting on a chair with the painful leg stretched out, pressure or movement causing unbearable pain. With difficulty he hobbled to my house and when he got into my room described the pain as "something awful." There was a distinctly painful point near the middle of the thigh. Using the full force of the battery, thirty cells, I applied the negative sponge outside the tuberosity of the ischium over the sciatic nerve where it emerges below the pyriformis muscle, and the positive sponge to the popliteal space; thus including the painful part between the sponges. He declared he could not bear the pain of this, but I persisted for nearly five minutes with the application, my patient roaring with pain the greater part of the time; when I removed the sponges he was astonished at finding the pain had gone, he pressed on the part that had been so painful and kicked out his leg to make sure that there was no mistake about it, and finally walked home without a limp. There was a slight numbness in the leg next day; but since then there has been no return of the pain.

Case 7.—Mrs. F., æt. 42, mother of a large family, has been for years a martyr to sciatica; in May last she consulted me about it, though she said she could scarcely hope for relief. I applied the continuous current to the same parts and with the same force as in the case last mentioned, and with the same result. In a few days, however, she came again, as the pain had recurred, and again she got immediate relief. Two days after this second application I gave her a third, and since then she has been quite free from pain.

Headaches.—No one who has not tested for himself the value of electricity in various kinds of headaches can form even a proximate estimate of the relief that it constantly affords. Its beneficial effects are most apparent in those aggravated and

persistent attacks of headache which have resisted all ordinary treatment. Hemicrania or sick headache is specially amenable to the continuous current; a few gentle applications, and in some instances one application, from two to four cells of Stöhrer's continuous current battery, the sponges being applied to the temples or mastoid processes, for two or three minutes, will effectually give relief. I refer as an illustration of the effect of electricity in relieving headaches to the following case, not only on account of the severity of the headache, but because in it marked relief was afforded to intense pain in the eyes and intolerance of light (photophobia) symptoms of the glaucomatous affection that existed in them. Photophobia is relieved by the continuous current, not only when it results, as in this case, from glaucomatous disease, but also from its more constant cause, ulceration of the cornea.

Case 8.—Intense headache, pain in the eyes and photophobia relieved by the continuous current.—A married lady sent for me in consequence of the pain she was suffering in the head and back of the eyes; the pain she described as terrible, and “as if something was being bored through the temples.” She said her eyes felt as if they would “crack,” and she could not bear the light. This lady, three weeks previously, had consulted me about her eyes, and the rapidly increasing loss of sight which she latterly noticed had been coming on. Ophthalmoscopic examination then revealed far-advanced glaucomatous excavation of the optic disc of one eye; the dilated retinal veins characteristically curving over the cupped edges of the disc. Atrophic changes were apparent in the other eye. With a view to ascertaining the advisability of submitting to iridectomy, I advised this lady to consult an eminent oculist. Weeks, however, elapsed, and when I now saw her she was prostrate with pain. Finding the morning after my visit that no relief had been obtained by the remedies I had prescribed, I applied the continuous current, using four cells, the sponges being applied to the temples and also to the mastoid processes, and the application lasting four minutes. At the expiration of half an hour after this application my patient said that the pain in the head was quite gone at the right side, and much less at the left, but the pain in the back of the eyes was still severe. I then applied the moistened sponges to the closed lids of either eye and the corresponding temple, the application for each side lasting two minutes. She was greatly relieved by this, and in the evening told me that the pain was very slight, more like stiffness than pain, but that the tight feeling in the eyes continued. That night she took pot. bromid. 10 grs., chloral 15 grs.

The next day I found the right side of the head and right

eye were perfectly free from pain, but there was a heavy painful feeling in the left. Most complete relief was then afforded by another application, and the photophobia disappeared. The pain having in a degree recurred during the night, I applied the galvanism again next day, affording immediate relief.

Rheumatism.—In the treatment of local rheumatism no remedy has more decided and permanent effect in relieving pain than the judicious application of electricity. The relief afforded by the subcutaneous injection of morphia is too often only temporary, while ordinary medicinal treatment most frequently altogether fails. In using electricity for muscular rheumatism, I have found that a combination of galvanism and Faradisation is the most certain mode of securing relief from the pain. In many cases I found that the induced current alone did not ease the pain; but if applied after a longer or shorter course of galvanism, its beneficial effects were at once apparent; while in many cases galvanism failed to give relief till the induced current had been subsequently applied.

Myalgia.—It is important to distinguish between myalgia and muscular rheumatism; Dr. Inman, of Liverpool, has drawn attention to the subject, and has described a train of symptoms sufficiently distinctive and well marked in the following case.

Case 12.—*Myalgia of month's duration cured by six applications of continuous current.*—Mrs. B., æt. 52, came under my care in May last; she had been for some months attended by a homœopathic druggist for bronchitis (?); when I saw her she was reduced to such an enfeebled state that she could not walk without support on either side; the body appeared exsanguinated; she was reduced to a skeleton, and general debility was extreme; decided asthma was present, and well-marked emphysema of both lungs. For weeks past she had not slept; unable to lie down, owing to the difficulty of breathing, she spent the night in trying to move about her room, as she fancied that by doing so she got some relief from the burning pain which tormented her in the back of the neck whenever she attempted to sit still. In the arms and legs she constantly suffered from severe cramps. She had been taking antimony, and had been kept on low diet. Quinine, iron tonics, good nutriment, wine, and rest procured with chloral and bromide, did much to restore her strength, but the pain in the neck resisted all the liniments I could think of—chloroform, belladonna, aconite, &c.; this always returned with renewed force when she had been up for a few hours. As soon as she had gained sufficient strength to come to my house I applied the continuous current, from first four and finally eight cells; I applied it altogether six times. The pain always left imme-

diately after each application, though at first it returned towards night; but after the fifth and final application it did not recur. I have just heard from the friends of this lady that since that time her health has steadily improved, and that there has been no return of the pain in the neck; moreover, and this is a significant fact, there has been considerable relief to the breathing, and she has not since then had an asthmatic attack.

Diseases of the Digestive Organs.—Of all painful diseases, there are none in which the relief afforded by electricity is more constant and striking than in diseases of the digestive system. In obstinate cases of vomiting, constipation, dyspepsia, flatulency, persistent hiccough, gastrodynia, acid eructations, &c., I have found both galvanism and Faradisation of great value, and in many cases after years of suffering had been endured. The following cases are selected from a large number, because in them actual pain was the most prominent symptom and the one for which relief was sought.

Painful Dyspepsia.—*Case 13.*—*Pain in stomach and bowels, vomiting, flatulency, of seven years' standing, cured by the continuous Current.*—Mr. —, æt. 65, for seven years had been a martyr to pain in the stomach and bowels; the pain was always increased by taking food, and was only relieved by vomiting or the discharge of quantities of gas by the mouth; there was obstinate constipation, which had to be overcome by strong purgatives; the paroxysms of pain were at times so intense, that the man has been "thrown down in an agony," and on one occasion, his employers believing that he was intoxicated, very narrowly escaped rough treatment. He was very temperate, and his diet was very limited; he had learned to avoid those articles which increased his discomfort. Of medicine he had certainly taken his share; his occupation compelling him to change his residence occasionally, he had the benefit of much and varied medical skill; indeed, he said that for seven years he had never been out of the doctor's hands. I prescribed Friedrichshall water to be taken in the morning, and Vals water with a little brandy to be taken during the day. I at once began by galvanising the bowels, applying the positive pole to the rectum, and passing the other sponge from spot to spot over the whole of the abdominal walls, and using a considerable amount of pressure. I began with twelve, and ended with thirty cells, and each application lasted five minutes. As the sponge was passed along the track of the colon, the motion of the wind was apparent, and the noise distinct; I also galvanised the stomach itself, by pressing the negative pole on the cardiac end, and passing the second sponge with deep pressure towards the pylorus. This treatment I continued for six weeks, two applications each week.

After the first application the pain was never severe, and vomiting never occurred; at the end of the third week wind was freely passed by the rectum, and the Friedrichshall was quite sufficient to produce a comfortable daily stool. When I discontinued the galvanism there had been no pain whatever for several days, and a month after he visited me, saying he was a new man, and that his object in coming was to obtain leave to extend his dietary. I saw this patient again to-day, Nov. 29th, and his report is in every way satisfactory; his morning draught of Friedrichshall is his only physic; the pain has never returned.

Case 14.—Dyspepsia, pain in stomach and flatulent distension of bowels, relieved by continuous current and Faradisation.—Mrs. —, about 45 years of age, and mother of several children, had for years been a great sufferer from pain in the stomach and bowels, which was always worse after taking food; there was a great flatulency and distension of the stomach, causing at times distressing irregularity of the heart's action. The abdominal parietes were very weak, and the bowels were extremely constipated, owing to the feeble peristaltic actions of the intestines. This lady was so circumstanced that she had had the advantage of the very best medical advice that could be procured, but any relief she got was only temporary, and when she consulted me looked on any cure for her sufferings as hopeless. I suggested electricity, which was readily assented to, but the accompanying gentle medical treatment which I prescribed was obstinately resisted. I galvanised the stomach and bowels, applying the sponges in the same manner as in Case 10, and using from eighteen to twenty-six cells. After five applications the relief to the pain was complete and permanent, but subsequently I Faradised the abdominal muscles with marked benefit. I have recently seen this lady, and she says that, remembering how long and wearily she suffered, she looks on the effect of the brief galvanic treatment I had subjected her to as something miraculous.

Case 15.—Flatulency and painful dyspepsia relieved by the continuous current.—Mrs. —, æt. 59, a corpulent woman, and mother of a large family, consulted me on account of pain in the stomach to which she had been subject for years; flatulence was also very painful and distressing, and she constantly vomited quantities of watery fluid. Her diet was restricted to a few articles of the simplest nature, and the quantity of food she took was extremely small, and to this she attributes the great weakness of which she complained; when I first saw her she was able to walk but very short distances. The bowels were constipated, and wind seldom passed by the rectum. She had taken quantities of medicine, charcoal, bismuth, pepsine,

&c., but nothing gave her relief. I prescribed Pullna water to be taken in the morning and Vichy during the day, and galvanised the stomach and bowels; she was obliged to leave Blackpool for a time, but returned, and I continued the treatment. I applied the continuous current, using twenty cells, altogether eight times, and subsequently Faradised the relaxed abdominal muscles several times till her strength was restored. When this lady left for her home she could eat heartily with comfort and relish, take long walks, and had been for some time free from pain.—*Liverpool and Manchester Med. and Surg. Reports*, 1876, p. 164.*

19.—ON PSEUDO-HYPERTROPHIC PARALYSIS (OF DUCHENNE.)

By Dr. WILLIAM H. BARLOW, Honorary Medical Officer,
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The interesting and curious disease which forms the subject of my paper was first described by Dr. Duchenne (de Boulogne) in the year 1861, in the second edition of his admirable work on "Localised Electricity," published in Paris in that year. As he himself relates ("Archives Générales de Méd.," 1868), the discovery of this affection dates from the year 1858, when a child, which had been struck with a paralysis of an unaccustomed form, was sent to his civil clinique by his friend M. Bouvier. "Having collected," he goes on to say, "in three years certain facts absolutely similar, and to which I did not know anything analogous in science, I believed myself entitled to consider that muscular affection as a morbid species not yet described and peculiar to infancy."

The name first given to this disease by Duchenne was "Paraplégie hypertrophique de l'enfance, de cause cérébrale;" he has, however, since then found cause to alter his opinion, and is content now with the name he places at the head of this treatise in the "Archives Générales de Médecine." Jaccoud calls it, and with him agree most English and American writers, "Sclérose musculaire progressive," after Requin, progressive muscular sclerosis. Heller proposed to call it "Lipomatosis musculorum luxurians," and many other names have been proposed by various writers; but the two which I have used in the title of this paper seem to me to be the best and most descriptive, the former of the prominent and striking feature to the outward observer, and the latter of the pathological muscular change.

Symptoms.—The principal morbid phenomena are, as Dr. Duchenne gives them—

* [For electric batteries of all kinds we can recommend Mr. Elliott, of Leighton Lane, Leeds, who makes them well, and at a very moderate cost.—Eds.]

1st. In the beginning, feebleness of the lower limbs.

2nd. Lateral balancings of the trunk and widening of the legs during walking.

3rd. A peculiar curvature of the spine (ensellure), or saddle-back (lordosis), both in walking and standing.

4th. Equinism (talipes equinus), with a peculiar over-extension of the first phalanges of the toes, which Duchenne calls "griffe des orteils."

5th. Apparent muscular hypertrophy.

6th. Stationary condition.

7th. Generalisation and aggravation of the paralysis.

When the disease has arrived at the stage of apparent hypertrophy, the appearance of the patient is very characteristic, and its true nature would be at once obvious to any one who had any knowledge of its symptoms; but in the earlier stages there is but little to guide us to a diagnosis unless we have some hereditary history. Of the hereditary nature of this affection the published cases give ample proof.

The children are at birth apparently strong and healthy, and it is not until they arrive at the age of twelve or fourteen months that anything is noticed. At that period it is found that when they are placed upon their feet they fall down, or clutch at the nearest object to support themselves; or in other cases it may be that the child has commenced to walk, when without pain or fever, or sometimes after convulsions, it is found to be soon fatigued, either by walking or standing, and at length it can no longer walk or hold itself upright; or again, it may be that the child does not walk until very late, $2\frac{1}{2}$ or 3 years, and then very feebly and imperfectly. In one of my own cases the child did not begin to walk until 2 years old, and then but very badly, and would hold on by the chairs or any object within reach, and in another it had never walked well, but, as the mother said, "always walked wide and so dropped down."

These defects in movement do not excite so much attention among the class from which hospital cases are usually taken, as the limbs are apparently well developed, and they are constantly hoping to see the child recover from what they imagine to be a temporary infirmity. When the child so falls, it raises itself by the aid of the nearest article of furniture or other object that it can reach, or if none be within reach then in a very peculiar and characteristic manner. When the body falls forward these children apply their hands successively to the legs and thighs and so as it were push the body upwards, and when attempting to mount the stairs they drag the body upwards by the arms. The mode of progression is by a waddling, "duck-like" movement ("dandinement"), alternate

balancing of the body on the leg which is applied to the ground. The equilibrium is always unstable both when walking or standing, and a slight force will cause the patient to fall "all of a heap;" in the case reported by Dr. Foster the boy had to be removed from school because the other boys would push him over, in order to see "his grotesque and futile efforts to raise himself." In the efforts to walk or in standing alone the feet are widely separated.

"It is true," says Duchenne, "that lateral balancings of the trunk are observed also in the child who begins to walk normally, but they are not very manifest and soon disappear, whereas in the disease I describe they increased and continued during the disease. I have never seen this symptom in children attacked with other paralyzes, and look upon it as one of the principal characteristics of the pseudo-hypertrophic paralysis." This peculiar gait and station he attributes to feebleness of the middle and small glutei ("fessiers"). As the case advances it is found that the child has a difficulty in bringing the heels to the ground; there is a condition of talipes equinus or equino-varus, and the toes are in a state of exaggerated extension upon the metatarsal bones; this condition of course increases the difficulty and peculiarity of gait. Before this latter condition becomes very marked, another and more striking characteristic becomes developed, in the form of a peculiar curvature of the spine, so that the shoulders and upper portion of the vertebral column, are carried backwards until a plumbline, falling from the most prominent of the spinous processes falls quite clear of the sacrum. This produces a deep curvature in the lumbar sacral region, a condition which Duchenne calls "ensellure," or, in English, saddle-back, and is due, he thinks, to weakness of the extensors of the spine, and is one of the most constant symptoms of the disease in question. In his monograph he gives plates contrasting this condition with that of the curvature produced by unopposed action of the spinal muscles, as in paralysis of the abdominal walls, where the curvature is at first glance somewhat similar, but the plumbline falls well within the line of the sacrum. (In the plates which accompany this the children are not standing alone, and therefore this is not so perceptible as it would otherwise be.)

The next symptom to appear is the one which is at once the great distinction and marked peculiarity of this affection—the apparent hypertrophy of the muscles. Its appearance is very gradual, but when the cases have advanced to the full development, the little patient may, as one of the cases pictured by Duchenne does, present the appearance of an infant Hercules. In most cases the calves and the buttocks are the chief seats of its manifestation, but in some, notably in the one just mentioned,

it may affect other muscles. Says Duchenne speaking of this case, "with the exception of the pectorals, of the great dorsal, and of the sterno-mastoids, it had invaded all the muscles of the limbs, of the trunk, and even those of the face, especially the temporals" (see figs. 3, 4, 11, "Archives Gén. de Méd.," 1868). The muscles stand out so prominently under the skin that Duchenne uses the term "hernial protrusion" to describe their appearance. There is nothing more astonishing upon first sight than this curious contrast between the exaggerated appearance of muscular strength and the marked feebleness which is the reality. The unaffected muscles are usually meagre and feeble, but the degree of impairment does not always coincide with that of enlargement; nor does every enfeebled muscle increase in volume. "I have seen," says Duchenne, "subjects in whom the feebleness of the muscles was more or less generalised, almost from the commencement, but where the hypertrophy was limited to the muscles of the buttocks and the lumbo-spinals." This condition of apparent hypertrophy continues generally for a much longer period than the group of symptoms previously narrated; and it forms the second, or stationary period of Duchenne, which continues advancing very gradually and almost imperceptibly, during two or three years, sometimes more (see Duchenne, "Electrisation Localisée," 3rd ed., p. 606, Paris, 1872). It does not necessarily begin in the lower extremities, though it generally does so, and extends gradually from these to others, and becomes localised in certain among a number of equally enfeebled muscles. "In one of my observations," says our author, "the deltoids had commenced to augment in volume many months before the gastrocnemii" (*loc. cit. supra*).

The people in charge of the patient in this stage, whether parents or nurses, are deceived by the false appearance of good development; and although they see the weakness and failure of muscular power, hope constantly for improvement—a hope which is seldom dissipated until the case enters after a more or less protracted period, upon the third and last stage of the disease.

In the preceding period there has been but little, if any, extension of the paralysis; the progress has been marked rather by increase in the volume of the affected muscles, than by changes in innervation. But this is now no longer so; the enfeeblement of the lower limbs increases, until the patients can no longer carry themselves upright; the upper limbs become paralysed, "*à leur tour, en masse*;" and unlike those attacked in the earlier stages, they do not increase in size; on the contrary, both these and those previously enlarged now rapidly diminish, and some of them become atrophied. Eleva-

tion of the arms becomes impossible, and the affected muscles lose gradually all that remains of motility, until the poor child, then generally arrived at adolescence, can only remain seated or recumbent. This condition may last for a considerable time. "Pseudo-hypertrophic paralysis," continues Duchenne, "is an apyretic malady in which the functions which preside over digestion, respiration, and circulation, are accomplished normally. The little patients may live a long time yet; at last they fall into a condition of great weakness, and are rapidly carried off by some intercurrent malady."

In one case the muscles were profoundly altered and presented to the touch the sensation of a pasty mass, non-elastic, and were traversed by yellowish striæ of a whitish-yellow tissue, showed everywhere a greasy reflection when cut. The electro-contractility was normal, and nothing was noticed in the nervous or vascular system differing from the normal. The patient was a boy of thirteen years. Duchenne himself denies that the surrounding fibroid interstitial tissue is composed of empty sarcolemmæ ("Electrisation Localisée" Paris, 1872, 3rd ed., p. 604), and in my own case there were certainly no such appearances.

The waxy fibres which surround the muscular fibres, and which contain a considerable quantity of ordinary adipose tissue (not oil-globules, but fat-cells), he looks upon as an increase of the ordinary connective tissue of the part, due to changes in the nutritive force, but the appearances are perhaps best given in his own words; he says ("Electrisation Localisée," 3rd ed., p. 603)—

"1. The hyperplasia of the interstitial connective tissue, with production of a more or less abundant fibroid tissue, is the fundamental anatomical lesion in pseudo-hypertrophic paralysis."

"2. It has its seat in all the paralysed muscles as soon as they commence to increase in volume, and this it is which justifies the anatomical denomination of myo-sclerotic paralysis, which I have proposed to give to it, and, with regard to its symptomatologic denomination, the pseudo-hypertrophic paralysis."

"3. This it is which produces the considerable augmentation of volume of the muscles, by reason direct of the increased (hyperplasié) quantity of interstitial fibroid and connective tissue."

"4. The increased (hyperplasié) connective and fibroid tissue is ordinarily mixed, or united to a small or mean quantity of adipose vesicles; indeed (ou bien), according to the facts observed in Germany, it is replaced by a considerable quantity of these adipose vesicles. This last condition appears to me

to be the most advanced degree of the interstitial muscular tissue in the pseudo-hypertrophic paralysis."

"5. From my personal observations the transverse striation is preserved in all the length of the most part of the muscular fibres, but it becomes extremely fine and very little apparent. In the parts where the transverse striation has disappeared, we see the longitudinal striæ; some of these longitudinal striæ are themselves effaced, the sarcolemmæ seem to contain adipose vesicles, which, in reality proceed from the surrounding interstitial tissue, and which, moreover, differ from the characteristic fatty granulations of fatty muscular degeneration."

"6. The hyperplasia of the interstitial connective tissue does not appear in general until the second period of the malady; it seems to me, to be preceded by an inflammatory state of the muscles, which may also occasion a slight augmentation of their volume. In this period (first period of the disease) the transverse striation is often of an extreme tenuity."

Duchenne believes that the paralysis is due to a lesion of the vaso-motor system, and calls to witness the alterations of capillary circulation and temperature, though no lesion of the ganglionic system has been as yet demonstrated.

As to the prognosis, the general opinion may be stated to be that in the first stage, before the muscles begin to enlarge, the prognosis, though grave, is not fatal; but that with the second period (that of enlargement) that hope leaves us; and still more is this so in the third or period of atrophy and debility.

The treatment includes the usual hygienic measures—baths, frictions, and shampooing, the iodides of iron, of potassium, strychnia, or nux vomica, and where there are cerebral symptoms the bromide of potassium has been found very useful. Tonics are indicated, arsenic, cod-liver oil, and Parrish's chemical food (the compound syrup of phosphates), have also been used with benefit, and probably free phosphorus in some of its therapeutic forms might be of value. But of all means within our power, the most promising is the prolonged and careful use of electricity in the forms of the induced and the galvanic current. I have myself used both forms on the same patient, and although both my patients, being out-patients, are rather irregular in their attendance, yet I believe that I can see some improvement. The instruments used are Stöhrer's batteries, and I have not used more than six cells of the constant battery, and his battery with one cell, for the induced current.—*Liverpool and Manchester Med. and Surg. Reports*, 1876, p. 1.

20.—ON SLEEPLESSNESS.

By Dr. J. MILNER FOTHERGILL, Assistant Physician to the West London Hospital.

[After reviewing the different forms of sleeplessness, Dr. Fothergill passes on to consider the chief forms of hypnotics in common use.]

To take opium first. Its use is rather indicated in conditions of insomnia which take their origin in pain. When there is vascular excitement present, it is desirable to combine with it direct depressants of the circulation, as aconite or antimony. The subsequent cerebral anæmia induced by the resort to opium is not so pronounced as is that induced by chloral.

Hyoscyamus takes its place alongside of opium, and may be resorted to in cases where opium or morphia disagrees, as in cases of chronic renal disease. For these last class of patients the tincture of hop is often very serviceable, though now rarely prescribed; it is a very satisfactory agent in such cases.

Hydrate of chloral is comparatively valueless in sleeplessness due to pain, and is inferior, in this respect, it is said, to the croton-chloral-hydrate. It is, however, very useful in conditions of vascular excitement, either alone, or in combination with opium. In the delirium of acute pyrexia in children it may be usefully combined with the bromide of potassium. In cases of sleeplessness where there is a sustained high blood pressure, or where there is distinct pyrexia, chloral hydrate is the hypnotic *par excellence*. It is, however, decidedly to be avoided in cases where the inability to sleep is due to worry and to brain exhaustion. In such cases, as in melancholia, the cerebral anæmia which follows its use is most objectionable and mischievous. It amounts to "brain-starvation," in fact, and the persons so affected are reduced to a pitiable condition. The persistent resort to chloral-hydrate is most disastrous in its consequences, and the temporary relief afforded by it is not to be set against its after effects.

Bromide of potassium has a decidedly sedative effect upon the brain cells; and the cerebral anæmia produced by its administration is rather due to its sedative action upon the cerebral cells by which they attract less blood to themselves, than to its effects upon the circulation; though doubtless to some extent it does diminish the activity of the heart. Its special advantage lies in its utility, where cerebral activity is kept up by far away peripheral irritation, especially when that irritation lies in the pelvic viscera. It may be given alone, or with opium, or with chloral, according to circumstances; and may often be usefully combined with hyoscyamus in cases where opium is contra-indicated. Its constant use, however, leads to diminished brain activity, and to intellectual lethargy.

Chloroform is a most potent agent, and is rarely resorted to as an hypnotic until other means of attaining the desired end have failed. The dangers attendant upon its use are so great that it is only resorted to in dire necessity. It is, however, occasionally used as a narcotic by the profession, but more frequently by persons upon their own responsibility. This chiefly occurs in those subject to sudden and unendurable pain, when nothing but the narcosis of chloroform would be effective. Probably indeed in these cases, all other and less objectionable means of attaining relief have been tried and have failed. According to Claude Bernard, by combining opium, or rather morphia with chloroform, the sensory nerves and centres are affected ere the intelligence and the motor powers are much influenced. But with chloroform alone all are equally and alike affected. The danger of chloroform inhalation lies chiefly in the risk of an overdose being taken; as unconsciousness creeps on the motor power is involved, and then the amount taken may be, and too often is, far beyond what was intended. In another communication in the *Practitioner* will be found some account of a most ingenious apparatus, by which the supply of chloroform is cut off as soon as the motor power is impaired. If resort to chloroform inhalation cannot be avoided by certain sufferers, surely it is not objectionable from any point of view that the danger attendant thereupon be reduced to a minimum.

There is another hypnotic agent of undoubted potency, which cannot be overlooked in the present inquiry, and that is—alcohol. If there be any use of alcohol that is free from objection it is its use as a narcotic in certain conditions. With many persons a dose of alcohol at bedtime is the very best nightcap they could possibly resort to. The cases best adapted to its use are those where there is mental worry and anxiety. In such states the first effect of alcohol in removing gloom and substituting pleasing sensations for unpleasant thoughts is eminently useful. A series of pleasant mental images are brought up on the mental horizon by its means, in place of the *triste* and sombre subjects which before its use occupied the foreground of the consciousness; and with such agreeable objects uppermost, the secondary effects come on, and the patient is wrapt in a refreshing renovating sleep. Probably the evil after effects of alcohol, so used, are less than those of any other agent which would achieve the same end. Unfortunately, however, commonly the very persons for whom alcohol would form the best hypnotic are those most opposed to its use; and where a full dose of alcohol would constitute the best remedy that could be resorted to, prejudice prevents its employment.

So much for the ordinary narcotic agents in common use.—*Practitioner*, Feb. 1876, p. 113.

21.—ON THE APPEARANCE OF PARALYSIS ON THE SIDE OF A LESION IN THE BRAIN.

By Dr. C. E. BROWN-SEQUARD, F.R.S.

Since the time of Mistichelli's discovery (in 1709) of the decussation of the anterior pyramids of the medulla oblongata, it has been universally admitted that one lateral half of the brain is the centre, or contains the centres, for the voluntary movements in the half of the body on the opposite side. Very strong—I may say decisive—arguments can be arrayed against this theory. On the one hand, either the anterior, the middle, or the posterior lobes of the brain, or a great part, or very nearly the whole of one hemisphere, or also a very great part of a lateral half of the pons Varolii or of the medulla oblongata, can be destroyed without paralysis; while, on the other hand, a paralysis may appear in one limb, or in both upper or both lower limbs, when the lesion is only in one lateral half of the brain. In other cases, and this is the principal object of this lecture, a paralysis may appear in one or in both limbs on the side of the lesion of the brain. If the universally admitted theory were true, most, or at least many, of the above-mentioned facts could not have existed; and if the new views first originated by Fritsch and Hitzig were true—if, in other words, special centres existed, one for the movements of the arm, one for those of the leg, many of the above-mentioned facts could also not have existed.

What I have just said of centres can be said of conductors also. If the conductors for the voluntary movements of the limbs on one side passed through the other side of the crura cerebri, the pons Varolii, and the medulla oblongata, a deep alteration, or a destruction, of a great part of one-half of these organs would *always* be followed by a paralysis of the limbs on the other side. It is far from being *always* so.

The same thing may be said of the face and the tongue. A lesion in one half of the brain above the pons Varolii can produce a paralysis of the face or tongue, either in the corresponding or in the opposite side, and there is no relation between the intensity and extent of the facial or lingual paralysis, and the degree, the seat, or the extent of the disease.

On the one hand, therefore, there is not *always* what we should find were the universally received theory quite true; and, on the other hand, there are a great many facts in absolute opposition to the theory, as regards not only the movements of the limbs, but also those of the face and the tongue.

That this theory must be rejected is not, however, the only necessary conclusion from the facts, the existence of which I assert; another conclusion also issues forcibly from these facts.

It is that one-half of the brain is sufficient for the movements of the two sides of the body. It is not without the greatest reluctance that I have come to admit the correctness of this last view—reluctance assuredly well grounded in a practitioner seeing every day cases of paralysis occurring in the limbs, the face, or the tongue on the side opposite to that of the lesion in the brain. I need not say that I felt at once that to put down the old theory and to replace it by mine, it was not sufficient to have facts against the universally admitted doctrine; it was essential also to explain how paralysis can appear indifferently in one or in the other half of the body from a lesion in the same part of the brain.

In the actual state of science the cases of paralysis on the side of the lesion of the brain are looked upon with very dubious eyes. Dr. James Copland denies almost absolutely that there are such cases. He states that the lesion found may have existed without producing paralysis, and that the paralysis that was observed may have had its cause in a lesion in the opposite side of the brain, not found after death. I do not doubt the possibility, even the probability, that it was so in some cases, but I will prove that, at least sometimes, the brain-lesion found on the side of a paralysis was the real cause of that loss of voluntary movement.

Before examining clinical facts, I will say that experiments on animals have given me this unforeseen result, that *a paralysis on the side injured always follows certain injuries of the surface of the brain*. The lesion which has the greatest power to produce this direct paralysis consists in a burn of a part of the surface of one hemisphere of the brain. The anterior, the middle, and the posterior lobes possess the power of generating that paralysis, but the two last parts more than the first. The deep-seated parts of the brain—*i.e.*, the walls of the lateral ventricles, the corpus striatum, the optic thalamus—have also that power. The paralysis so produced is very slight, and it is variable as regards its seat: it usually exists in one of the limbs on the side of the lesion, sometimes in both; it also appears sometimes in the abdomen, the thorax, the face, the neck, the bladder. If the burn of the surface of the brain is deep, and very extensive, it produces sometimes a considerable paralysis in the four limbs, more intense in the two abdominal limbs, or in the two thoracic limbs, and greater, in those instances, in the limbs on the opposite side than in those on the side of the brain-lesion.

The irritation due to a burn of the surface of the brain in rabbits, guinea-pigs, cats, and dogs, is not the only cause able to produce a paralysis on the side of a brain-lesion. Long ago Méhée de la Touche found that a transverse section of certain

parts of a lateral half of the brain or of the cerebellum in dogs is sometimes followed by a paralysis on the same side. I have seen the same thing, but very rarely, and the paralysis has been less than that which was seen by the surgeon just named.

If a paralysis appears in dogs and other animals on the side of a lesion of the brain, we certainly can look upon the same thing as possible in man; nay, there is a probability that it sometimes exists. This being stated, let us see what are the facts observed in man in which a paralysis has, or seems to have, existed on the side of the brain-lesion. I may startle many of my hearers in stating that I have collected *more than two hundred* of such cases. Burdach, out of 258 cases of paralysis in one half of the body, states that there were 15 on the side and 243 on the opposite side of a lesion in the brain. W. Nasse, besides the 15 cases of Burdach, knew of 26 cases of paralysis on the injured side in the brain. In two good papers on the subject by Bayle and Dechambre, not more than 10 or 12 cases are pointed out. The 200 I have collected do not include those of Nasse's list (at least most of them), as I have not been able to procure the paper of that learned physician. But it is not the number of such cases that can alone carry conviction. The features of certain cases, or some peculiarity attached to them, will do much more to prove that the paralysis was really due to a lesion on the side of its appearance. The demonstration is, I believe, fully given by the following facts and arguments:—

1st. In a case in which a small bullet (a pistol shot) passed through the whole *right* side of the brain from back to front in the upper part, and at a distance of an inch from the middle line in the occipital bone, and one-third of an inch in the frontal bone, there was at once a paralysis on the same side (the *right*). Eight months after the paralysis was not quite cured. The case has been reported with great care by two Belgian physicians, Drs. Liévens and Demoor. It is impossible to admit that there was anything here but a lesion of the three lobes of the right side of the brain in parts where there is no large bloodvessel and above the lateral ventricle, so that no great effusion of blood can have taken place, and the effused blood cannot have passed from one ventricle into the other.

In a case in which a large mass of medullary cancer involving the *left* parietal bone had made its way outwardly, a pressure upon the tumour “caused temporary loss of power of motion in the *left* arm—that is, the arm of the affected side.” Mr. George W. Callender, who published this case, has supposed that in cases of paralysis on the side of a lesion in the brain there was a pressure exerted on the opposite side, the paralysis resulting from this pressure. In this case it is clear that a pres-

sure on the tumour, if able at all to act on the right side of the brain, must necessarily have spent almost all its force on the left side, so that the temporary paralysis was evidently due to a lesion on the same side as that where it occurred.

In a very interesting case, published by Scholz, of Bremen, a patient was attacked with aphasia, drooping of the *left* upper eyelid, and *left* hemiplegia, and was cured after the issuing of a good deal of pus from inside the cranium, through an opening, on the *left* side of the head at nine centimeters below the the sagittal suture and on the coronal suture. An effort has been made to explain this case in admitting that the pressure exerted by the intracranial abscess had produced œdema and anæmia in the opposite side of the brain. It seems wonderful to me that such an idea can have, even for a moment, come to the mind of a surgeon of merit! Is it necessary to show that if the abscess was located near the place of exit of pus it is the left half of the brain alone, or nearly so, which bore the pressure, and that if œdema and anæmia resulted from that pressure, it was in the left half of the brain that they occurred?

In another case it was clear also that the paralysis originated from a lesion on the corresponding side of the brain, as it came in the *right* limbs after a blow on the forehead and the temple on the *right* side; and the autopsy showed that the *right* parietal bone was perforated, and there was a large abscess over the *right* hemisphere.

In all the above cases the knowledge during life of a lesion on the side of the paralysis existed, and the connexion between the lesion and the paralysis was so obvious that it seems to me impossible that anyone giving time to the examination of these cases can suppose that an *unseen lesion* in the hemisphere of the brain on the opposite side to that of the paralysis was the real cause of this loss of movement.

Dr. W. Swayne Little, according to Gintrac, has reported the case of a patient whose *left* side became paralysed after having had the *left* anterior lobe of his brain pierced by an iron spit.

Abercrombie gives a case of Hill, in which, after two months, during which there had been vomiting, headache, and numbness in the *left* hand, the whole *left* side became paralytic. After another month a small tumour appeared by the side of the bregma, which discharged matter with some relief. Seven months later Mr. Hill found an opening in the *left* parietal bone, and, having applied the trephine there, he discovered an abscess, which discharged four ounces of matter. There was much relief after that discharge, but protrusion of the brain took place, and the patient died. The brain was found destroyed by suppuration two inches around the opening of the

skull. In this case it is clear that the paralysis depended on the abscess, as it increased with it, and it diminished twice after the issue of matter.

J. Lafargue gives a case of fracture of the *left* parietal bone, followed by *left* hemiplegia. Near the fracture the cerebral substance was found reduced to a brown-greyish pulp (*detritus*); this colour, becoming bluish, extended to the centrum ovale. The right hemisphere was perfectly normal. There can be very little if any doubt that in this case the cause of the paralysis of the left side of the body was in the left half of the brain.

2nd. The second reason I will give to prove that a paralysis may be caused by a lesion in the same side of the brain is drawn from cases in which there were symptoms of hemorrhage in the brain at the time that a paralysis appeared, and in which no hemorrhage in that organ was found after death except on the side of the paralysis.

Diday, a very accurate observer, relates that a woman lost consciousness suddenly and completely, and when she recovered her senses her right limbs were paralysed. For five years she remained incompletely paralysed (of the *right* limbs and *right* side of the face). She died of heart disease, and the autopsy showed no disease in any part of the brain, except in the *right* side. An old cyst replaced the right corpus striatum, and the neighbouring white radiations. In the *right* posterior lobe there was a very small yellow cicatrix.

Dégranges, quoted by Gintrac, gives the case of a girl who was attacked with vomiting, coma, paralysis of the *right* limbs and convulsions, then spasmodic contraction of the same limbs. In the centre of the *right* posterior lobe, a small clot was found surrounded by a yellow, soft pulp, with red punctuations.

Mr. G. W. Callender gives a case of sudden paralysis of the *right* side, followed by death in nine hours. A recent clot, the size of a walnut, was found in the *right* hemisphere. Thence the blood, passing through the corpus striatum, had entered the various ventricles.

A very interesting case, with considerable details, is given by Hillairet, a distinguished physician of Paris. The patient had several attacks of hemorrhage of the brain, all in the right half; the *right* posterior lobe was almost destroyed, and the *right* optic thalamus was notably atrophied. The paralysis was on the *right* side.

In a well-recorded case, Gintrac states that a patient became paralysed of the whole *left* side, face included, after an attack of apoplexy. There was a closure of the *left* eye, and diminution of feeling in the *left* arm. Between the corpus striatum

and the optic thalamus on the *left* side, and the cerebral substance on the external part of the organs, there was a cavity three inches in length, one inch in height, and as much in width, containing a clot of blood.

There are on record a number of other cases of apoplexy with hemiplegia on the side of the cerebral hemorrhage. I will only name the authors who have published such cases:—Morgagni, Brunner, Lermnier, Dr. R. Boyd, Rostan, Albi.

It is clear that when we find hemiplegia appearing together with apoplectic symptoms, we must admit that the only lesion found, and consisting in evidence of fresh or old laceration of brain-tissue by effused blood, is the cause of the paralysis; and if, as in the above cases, the loss of motor power appears on the side of the brain lesion, it is evident that in those cases the explanation that that kind of hemiplegia depends on an *unseen* lesion in the opposite side of the brain must be rejected.

3rd. In a number of cases of paralysis on the side of the only lesion found in the brain there was aphasia coexisting with left hemiplegia, the left side of the brain being the seat of the only organic alteration found after death. Such is what was found in the following cases. One of Professor J. Syme, in which there was fracture of the cranium, with very considerable serous effusion pressing over the *left* middle lobe, which was much altered; this lesion producing aphasia and paralysis of the *left* side. Another of Cruveilhier, in which a great part of the *left* anterior lobe was hardened and red, paralysis of the *left* side and loss of speech being the symptoms. A third of Bayle, in which there was disease of the meninges and the convolutions and white substance of the anterior lobe on the *left* side, with aphasia and paralysis of the *left* limbs.

Unless we admit either that the aphasia alone depended on the lesion found in the left side of the brain and the paralysis on a supposed but unseen lesion of the right side of the brain, or that both the aphasia and the paralysis did not depend on the only organic alteration found in the left half of the brain, but on another lesion not found, but supposed to exist in the right half—two equally improbable explanations,—we must accept as correct that paralysis in those cases was caused by a lesion in the corresponding side of the brain.

4th. The extent of the lesion is also a strong argument in favour of the view that in some cases of paralysis on the side of the organic alteration in the brain the cause of this loss of movement was that alteration itself, and not a supposed but unseen one in the other half of the brain. Among cases of that kind I will briefly mention some, two of which—those of Dr. Henry Day, of Stafford, and Dr. Dechambre, of Paris—

deserve special attention, on account of the great care with which the autopsy was conducted.

P. P. Broc relates that in the case of a woman who had lost the power of motion and feeling in the *left* limbs, remaining intelligent, the *left* hemisphere was reduced to a pulp (*bouillie*).

Freschi states that a woman who had once had a temporary paralysis of the *right* arm one day screamed out that her *right* arm and leg were dying away, after which she lost consciousness, and soon died. The left hemisphere was found perfectly healthy; the *right* was transformed into an enormous cavity containing a blackish clot and fluid blood.

Mr. G. W. Callender gives this case:—A male, after having been attacked with epilepsy, and then cured for five years, had a return of the fits, with *left*-sided paralysis. From front to back the entire *left* hemisphere above the level of the corpus callosum was one large abscess. A mere shell of soft brain-tissue separated it from the ventricles and from the surface.

Ch. Curtius speaks of a case of apoplexy, with paralysis of the *right* side, in which the whole *right* hemisphere was converted into a mucous substance, which came out like a thread when a knife was lifted up out of it.

Rostan, in a woman who had been suddenly attacked with a complete hemiplegia of the whole *left* side, found almost the whole *left* hemisphere in a state of pultaceous softening.

In an individual who had suddenly been attacked with complete paralysis of the limbs and face on the *left* side, Dr. Henry Day found that the *left* middle lobe from top to bottom, and including the corpus striatum, was completely softened and broken down almost into a pulp.

Dechambre, in a good paper on the subject of paralysis on the side of the lesion in the brain, gives all the details of a case in which a sudden paralysis, which soon became complete, with drooping of the upper eyelid, occurred in the *right* side. The autopsy showed no lesion in the left side of the brain, but there was considerable alteration of the *right* hemisphere. Many parts of the middle lobe were reduced to a reddish pulp, the corpus striatum was destroyed, and the softening occupied the centrum ovale and also parts below the lateral ventricle.

In the presence of such facts it seems to me almost impossible to maintain that the great lesion found at the autopsy of those seven patients had not produced paralysis, and that the hemiplegia existing depended on an alteration not detected in the side of the brain which was declared healthy by the able men who made the autopsy.

5th. The location of a lesion in the brain may give also a strong argument in favour of the view that a paralysis can be produced by a lesion in the corresponding side of the brain. If

we find, for instance, that simultaneously with a paralysis in one half of the body, symptoms appear which point to a certain part of the base of the brain as the seat of the disease, and if the autopsy confirms this diagnosis, it is almost certain that the paralysis was caused by the lesion found. I was able in two cases to diagnose the seat of the disease in cases of paralysis on the side of the lesion, and the autopsy showed that I was right.

In a very large number of cases paralysis has appeared on the side of the encephalic lesion, when it existed in or on the pons Varolii, the cerebellum, the crus cerebelli, the medulla oblongata, or other parts of the base of the brain. Indeed, *in cases of disease of the petrous bone or of the surface of the encephalon near that bone, paralysis, if it occurs at all, will appear as frequently on the side of the lesion as on the opposite side.* I will by and by discuss the question whether there are there nerve-fibres that have not yet made their decussation. All I wish now to impress on your mind is that those parts of the cerebro-spinal centres are very peculiar. They are so not only because they often give rise to paralysis on the side of the lesion, but also because they can be more injured than any part of the spinal cord—of which most of them are chiefly a continuation—without giving rise to symptoms, and because when injured in one of their halves there is either no paralysis or a paralysis on the opposite or on the same side, and in this variety also they greatly differ from the spinal cord.

Among the cases of paralysis on the side of the lesion, with disease of the base of the brain, there is one which must be singled out, as it shows more than any other that with the destruction of all (or very nearly all) the motor conductors of one side—those that some suppose not to have yet made their decussation, as well as those the crossing of which with similar ones of the other side is already made—paralysis can appear in the limbs and face only on one side, and that side the one in which the lesion exists. This case was long ago published by Mr. Stanley. The *left* side of the face of the patient was attacked with paralysis of motion and sensation; the *left* limbs were paralysed of motion. The autopsy showed that a tumour occupied the whole of the *left* side of the pons Varolii, extending into the *left* crus cerebelli.

If we now survey the whole field of the cases I have briefly mentioned or alluded to, it is clear that the following conclusion comes out forcibly from these facts—that there cannot be a doubt that there are cases in which hemiplegia has been caused by a lesion in the corresponding half of the brain. But is there no explanation of these cases in harmony with the universally admitted theory that one-half of the brain is the centre, or

contains the centres, for the voluntary movements of the limbs on the opposite side?

Direct, like crossed paralysis, may be due to the most various organic causes: a wound, a tumour, an abscess, softening (due to embolism or inflammation) atrophy, or hemorrhage. Direct paralysis, like the other kind, may also be caused by disease almost anywhere in the brain: the meninges, the convolutions, the anterior, middle, or posterior lobes, the centrum ovale, the walls of the lateral ventricle, the crus cerebri, the crus cerebelli, the cerebellum, the pons Varolii and the medulla oblongata, and also the corpus striatum and the optic thalamus. Direct paralysis can also, like crossed paralysis, be complete or incomplete, strike only one limb or both, be accompanied by contracture or convulsions, anæsthesia or hyperæsthesia.

Many persons erroneously believe that in our century cases of direct paralysis have been less frequently observed than in the preceding centuries; and this supposition is allied with the other, that at least many of such cases are worthless because the authors who have published them were not good observers. Besides the names of able or eminent men I have given already, there are many others, so that there is no doubt as regards the exactitude of observation. It will be enough to point out at random among the writers, not yet named in this lecture, who have published cases of direct paralysis the following:—Dr. J. Hughlings Jackson, R. Bright, Mr. Campbell de Morgan, Cruveilhier, Prof. Horner, Dr. Peter Eade of Norwich, Jobert de Lamballe, Wenzel, Dr. Reynaud of Paris, Prof. Lebert, Gibert, Cazauvielh, Prof. Andral, Blandin.

Cases of direct paralysis are certainly not so very rare as is believed. I have seen five or six—two only, it is true, in which an autopsy has demonstrated the correctness of the diagnosis. Wedemeyer, Rostan, Mr. Callender, have each of them seen many cases. My friend Dr. J. Hughlings Jackson has seen at least two, and many other observers have also published two cases.

Reverting now to what I have said in the beginning of this lecture, I will repeat that it is not possible to look upon paralysis in cases of brain disease as being the effect of loss of function of the part diseased, and that we must admit that in those cases there is an irritation starting from the place we find diseased after death, and acting on more or less distant parts of the nervous centres, in such a way as to arrest their activity, and thereby cause the paralysis.

With this view, and only with it, we can explain that a lesion in one part of the cerebral lobes will produce or not a paralysis, that the paralysis will exist in the same or the opposite side, or in both sides (and there are many such cases

in which a direct and a crossed hemiplegia simultaneously, or one after the other, will appear in consequence of a lesion in only one-half of the brain). With the view I have given, the immense differences that are found in the degree, the duration, as well as the seat of paralysis, can be explained, as we are to expect the greatest variety in effects of irritation, while this excessive variety is a death-blow to the admitted theory that paralysis in brain disease depends on loss of function of the part diseased.

These facts and reasonings, with many others mentioned in this lecture, as well as a great many not mentioned, lead to the following conclusions:—

1st. That it is wrong to conclude from the apparition of paralysis, when there is disease in the brain, that the loss of movements depends upon the loss of function of conductors or centres employed by the will in the production of movements.

2nd. That if there is any decussation of voluntary motor conductors anywhere in the base of the brain, it is not owing to the absence of such a decussation that direct paralysis sometimes appears.

3rd. That it is owing to an irritation that one-half of the brain is capable when diseased, of producing paralysis either in the corresponding half of the body or in the opposite one.—*Lancet*, Jan. 1, 15, and 29, 1876, pp. 2, 79, 160.

22.—TWO CASES OF NEURALGIA TREATED WITH CHLORIDE OF AMMONIUM.

By Dr. DAVID YOUNG, Florence.

The following two cases of neuralgia—one of the right ovary, and the other of the fifth pair—present several points of interest, and illustrate the value of the ammonium chloride in the treatment of these affections.

I. *Neuralgia of the right ovary of sixteen years' standing.*

Madame P., aged 37, widow, consulted me in January last. She gave the following history:—Married at nineteen; had three children, the youngest being sixteen—last confinement difficult and prolonged, but child born naturally. One month after she was seized with severe pain over the right ovary, which lasted three days. A fortnight later the menses appeared, and fourteen days after their cessation the pain returned in the old spot and continued about the same length of time.

For sixteen years matters went on in the same way. The monthly periods were regular and the discharge normal in quantity and duration. The pain came on invariably between the thirteenth and fourteenth day after the menstrual period

had ceased, and lasted from forty-eight to seventy-two hours. Sometimes the pain was not severe, but at other times so intense that it seemed, to use the words of Madame P., "like having a difficult confinement every month."

On examination the uterus was healthy and slightly anteverted; the right ovary was swollen and tender, and when examined per rectum was exceedingly painful. When I first saw the patient the pain had lasted about two days. I placed a pessary containing potass. bromid. gr. x. and ext. belladonna gr. $\frac{1}{2}$ in the posterior *cul de sac*, and ordered fifteen grains of the chloride of ammonium every two hours till three doses were taken. Next morning the patient was free from pain and had slept well. Nothing further was done till after the next period. I now used a pessary containing potass. bromid. gr. x. and ext. belladonna gr. $\frac{1}{4}$ every second night, placing it in the same position as before, and ordered fifteen grains of the chloride to be taken at eight, nine, and ten o'clock every night. The dreaded fourteenth day came, and with it only a very slight pain.

The above treatment was continued for two months longer, although the pain had not returned, and since then she has been perfectly well. After each monthly period during the course of the treatment there was slight tenderness, or rather uncomfortableness in the region of the ovary, which was relieved by the pessaries, and therefore I continued to use them every other night while the patient was taking the chloride.

II. *Neuralgia of the ophthalmic and superior maxillary branches of the fifth pair.*

Madame Van C., a widow lady, aged 50, awoke about three in the morning with severe pain over the left eyebrow and shooting down the cheek to the nose. Thirty years previously she had neuralgia on the other side of the face, which was cured by large doses of quinine. The present attack had lasted two days before I saw her, and I found her suffering intensely, and exhausted from want of sleep. I ordered five grains of citrate of quinine every two hours till she had taken thirty grains, which caused great headache, but no cessation of the pain.

On each occasion the pain set in with great severity between three and four in the morning, continued till about ten o'clock, and then gradually diminished till it finally ceased, about mid-day.

On the evening of the fifth day from the commencement of the attack the chloride was given in ten-grain doses every hour, from six p.m. till midnight, with the following interesting and satisfactory results:—

6th day, pain came on	3.30 a.m.	Ceased	one o'clock.
7th day,	4.0 a.m.	,,	one o'clock.

8th day,	„	5.0 a.m.	Ceased at noon.
9th day,	„	6.0 a.m.	„ noon.
10th day,	„	7.0 a.m.	„ noon.
11th day,	„	8.0 a.m.	„ a little before noon.
12th day,	„	9.0 a.m.	„ noon.
13th day,	„	10.0 a.m.	„ noon.
14th day,	„	11.0 a.m.	„ noon.

On the 15th day, the attack began about 11.30 and continued for an hour with more severity than on any previous occasion, and subsided a little before one o'clock. The chloride was continued for three days longer, and till the time when my patient left Florence, six months after the beginning of the attack. She had had no further trouble.

Note I.—The chloride, if given in larger doses than fifteen grains, is apt to cause nausea, and in some cases diarrhoea.

Note II.—If administered at distant intervals—say of three or four hours—the effect is much less marked than when given rapidly and in anticipation of the onset.—*Practitioner*, Dec. 1875, p. 412.

23.—CASES OF SCIATICA AND NEURALGIA SUCCESSFULLY TREATED BY GALVANISM.

By S. J. KNOTT, Esq., Medical Superintendent of Galvanism, St. Mary's Hospital; late Tutor and Pathologist, &c.

If there is any inflammation of the nerve, then galvanism must be used cautiously. I believe in all cases where the nerve is implicated that faradisation would cause great pain, and do harm. The great thing to determine is how the galvanism acts in these cases, because it does good in cases where several counter-irritants have been used, such as blisters, liniments, &c. It depends on the cause of the sciatica or the neuralgia how the galvanism is used, and if the cause is decided on, then the amount of galvanism applied would be, up to a certain amount, as a tonic and stimulant to a nerve which is asthenic; the cells used would be few, and the application light. But if the complaint has been caused by cold, sitting on a wet seat, gout, &c., and the nerve is tender when pressed upon, and the pain very acute, then the cells must be very much increased, and the poles pressed well into the part. The operation should be only for a few minutes, but severe. Sometimes around the cutaneous parts on which the charcoal points have been placed there occurs a herpetic eruption. If galvanism is applied in this way it is the most powerful counter-irritant possible, and the relief to the sufferer is very great.

Sometimes one has to treat cases of neuralgia caused by great debility, at other times shock, and often in two applications the

patients are well. Now, no medicines do this, and if any apparent benefit is obtained the stomach or some other organ suffers, as the head does in large doses of quinine. Then there is the morphia treatment by hypodermic injection. Now this line of treatment is all very well for the time being, to relieve the great pain and distress, but does it cure the malady? No. About five months ago five cases came under my notice where for months and months men had been treated with morphia injections. One man had got up to four grains and three quarters daily. If this kind of treatment is persisted in where is it to stop? the patient may as well be an opium eater. At last this particular patient got a large abscess in the gluteal region, from the irritation of passing the needle, I presume, and thus got rid of his sciatica. In the case of asthenic neuralgia I believe the mild application of galvanism not only stimulates the weak nerve at the time, but is of a lasting character, for I have often heard a patient say that the whole limb kept warm for two or three days after. I also believe that by this more or less continuous stimulation fresh nerve-cells are caused to be formed, and so the weak nerve is permanently strengthened. And the reason that faradisation is injurious in this class of complaints is that it causes contraction of the enfeebled nerve, and if used strongly a breaking up of nerve-cells, and great pain at the time, and most likely followed by an aggravation of the symptoms. If others have been as successful as myself in the treatment of this class of complaints by galvanism, then I hope it will be tried generally, and it certainly will be a great boon to patients to have this done, instead of taking the powerful medicines some do, and after all only to ameliorate their complaint.

B. G., aged 54, a surgeon, came under treatment on June 15th, 1873. His history was that two months previous he had got wet and was obliged to sit in his damp clothes for nearly two hours. When he got home he shivered a little and felt cold all over. He went to bed, but was called up in the night and had to go to a midwifery case, at which he stayed some time. On the following day he felt much out of sorts, and in the evening had some pain and stiffness down the back of the left thigh, which, in spite of everything he could do, gradually got worse. He then consulted a well-known physician, and he was put through all the usual modes of treatment, but with little effect; in fact, in spite of very large doses of quinine and aconite liniment, he used to get worse towards night. At the end of the fifth week the pain used to get very severe, and last for about two hours, every twelve hours. He then came to me as a last resource. He walked into my room supported by two sticks. The pain he complained of reached along the left

sciatic nerve, commencing just below the sciatic notch and extending to the popliteal space; he also began to feel pain in the loins.

I looked on this case as one of great congestion of the nerve, followed by effusion under the neurilemma; and the pain in the back as caused by the disease extending upwards towards the cord. I advised him to leave off all medicines so as to give the galvanism a fair trial, and to have recourse to the latter daily. I began with 18 cells of Stöhrer's battery. After the first application he stood up and said, "I believe this will do me good; the whole limb feels warm, which, in spite of wool, flannel, &c., it has not done for weeks." After the first week, I galvanised him only three times a week, then twice; and in three weeks he said he thought he was quite well—at any rate, he would see if he could do without the galvanism; he did so, and has been perfectly well since. He took no medicine from the day he first came, and at the end of the first week left off all the wool and flannel, and only continued his ordinary merino drawers.

[Mr. Knott relates several other most interesting cases.]—*Lancet*, Dec. 18, 1875, p. 872.

24.—NITRITE OF AMYL IN NERVOUS CEPHALALGIA.

By Dr. R. A. DOUGLAS-LITHGOW, North Brink, Wisbeach.

Some six years ago I had my attention first directed to nitrite of amyl, and as this therapeutic agent is at present receiving great attention from the profession, I wish to make known one of its effects which I have found almost specific; and I do so more especially as I am not aware that the action of nitrite of amyl in nervous headache has been recorded recently. As will be seen, I lay no claim to the discovery myself. I think it was in 1869, whilst reading a number of the *Edinburgh Medical Journal*, I observed a remark made by the late Sir J. Y. Simpson, in the report of one of the meetings of the *Edinburgh Obstetrical Society*. He said he had found the inhalation of two drops of the nitrite give immediate relief in cases of nervous cephalalgia, however severe. From that time to this I have been in the habit of so using it, and, where due care was taken in the selection of cases, I have never known it fail to produce entire and almost immediate relief. In several instances I have had to increase or repeat the dose, but, as a general rule, two drops have been sufficient.

I place two drops on the palm of the patient's hand, and quickly diffusing these with my finger over the palmar surface, I tell her to cover her mouth and nose with her hand, and to inspire deeply and quietly. No time should be lost after the

nitrite is dropped on the hand, as it evaporates rapidly. The patient should be seated while inhaling, as the peculiar effects of the nitrite are produced almost instantaneously, and may occasionally alarm a very nervous or hysterical female. Fortunately these symptoms last a very short time—generally less than two or three minutes,—and with their cessation the pain almost invariably ceases. Two drops may be given as a draught in water, instead of by inhalation, but I have found the latter mode much more satisfactory.

Dr. B. W. Richardson, to whom the profession owes almost everything concerning the true physiological action of this peculiar agent, says it is one of the most powerful relaxants of vascular tension we possess, and, bearing this in view, we can easily account for many of its physiological effects. I do not, however, propose to discuss these here, but I may just mention that, in addition to the “flushing of the face” which invariably results after the administration of nitrite of amyl, patients generally complain of great throbbing in the temples, “fluttering of the heart,” and a feeling of breathlessness, as if they were “dying away.” Some describe a sensation of “tingling from head to foot,” and several have experienced pains in the limbs analogous to cramp, while in other cases every object seems to have acquired a bright yellow hue, such as sometimes results from the use of santonine. These severer symptoms, however, are by no means common.

Used as I have just recommended, I don't think there is the slightest risk in its administration; but, owing to the temporary palpitation of the heart produced in most patients, care should be taken in administering it in cases of organic cardiac disease, &c. Should any excitement or other alarming symptom occur, cold affusion to the head, face, and chest, with a free supply of fresh air, are the proper means to use, but I have never had occasion to do so. The nitrite (which is cheap) should be procured from reliable makers only, and, as an economic suggestion, I recommend anyone using it to preserve it in a stoppered glass bottle, having an additional glass cap-cover.—*Lancet*, Oct. 16, 1875, p. 556.

25.—THE TREATMENT OF SUNSTROKE BY THE SUBCUTANEOUS INJECTION OF QUININE.

By Surgeon A. R. HALL, Army Medical Department.

The experience of several medical officers in India is now apparently sufficient to prove that the hypodermic injection of quinine in heat apoplexy is the most successful method of treatment that has yet been adopted. I have had conversations with several who have used it; and a friend of mine, a Surgeon,

major recently returned home, said to me:—"If there is anything in the practice of medicine which may be described as *magical*, it is the effect of the subcutaneous injection of quinine in sunstroke." Records of a good many recoveries have been published; but as a typical case, I select the following one recorded by Surgeon-Major T. C. O'Leary, M.B., Royal Horse Artillery, in the "Annual Medical Report of the Army for the year 1872." As I presume most of the readers of the *Practitioner* do not see this blue-book, I insert the case here in full:—

"A man of the D Brigade, Royal Horse Artillery, was, with others, unavoidably exposed in shifting baggage at the break formed by the Kistna river, on the line between the Madras and Bombay Presidencies. He was young, healthy, and had not been drinking. The train was about to start, at three o'clock in the afternoon, from Raichare, when he staggered up to the carriage set apart for the sick, and asked for admission, saying 'he was dying.' The bell had rung, and no time was to be lost. The water chatties were filled, a lump of ice procured, and the two medical officers accompanying the Brigade entered the carriage with him. The cold douche was assiduously employed, ice was applied at the nape, and friction of the limbs kept up. Though the temperature of the surface was brought to its natural standard, the patient was making no rally, and the heart's action was rapidly failing. Stimulants could with much difficulty be administered, as the jaws were firmly closed, and complete insensibility was almost established; in short, the case was becoming desperate.

"Quinine had been recently recommended in cases of this nature, and, as a syringe was at hand, it was determined to introduce the drug subcutaneously. The effect was closely watched, and after the first injection the pulse at the wrist was felt to flicker; this encouraged a further trial, and the result was so satisfactory that a third injection was accomplished. Within a few minutes the circulation was fully established, the man sat up, stared vacantly about, and recognized those standing near.

"It is only necessary to add, that within half an hour from the time when the first injection took place the patient was partaking of mutton broth, and was partly feeding himself. He soon fell into so comfortable a sleep that the medical officers were at liberty to leave him, and on the arrival of the train, at seven o'clock in the evening, at Goolburga, he was practically convalescent. On reaching Kirkee next morning he jumped out of the hospital carriage, and would have marched to barracks had he been permitted.

"The jolting of the carriage did not permit a nice calcula-

tion to be made of the quantity of quinine introduced at each injection, but the apothecary was of opinion that five grains, in all, must have been passed under the skin. No ulceration took place at the points of entrance of the needle."

During the month of May 1869 my friend, Mr. Walter Kerr Waller, of Calcutta, told me that he had been very successful in treating sunstroke by doses of 20 or 30 grains of quinine given by the stomach, and advised me to try it. A short time afterwards, I was called to see a driver of my battery, at Barrackpore, near Calcutta, who was a patient in hospital, and who was knocked down with heat apoplexy about five o'clock in the afternoon. I found him completely comatose, with dilated pupils, stertorous breathing, face very much flushed, skin of body red and *burning hot*, pulse full and rapid. I dissolved 20 grains of quinine in 20 minims of dilute sulphuric acid, and about 3 ozs. of water, and attempted to make him swallow it; but in vain. I therefore thought it a good opportunity for trying the hypodermic method. A solution of 5 grains of quinine in 5 minims of dilute sulphuric acid, and 50 minims of water, was put under his skin, in different places, about the shoulders. Within one hour, the heat of surface had perceptibly decreased; he steadily improved during the night, was quite sensible next morning, and recovered without any bad symptoms. As far as I am aware, this was the first case in which quinine was *hypodermically* employed. Surgeon J. Anderson, at present with the "Chestnut troop" of Royal Horse Artillery, shortly afterwards, in the same hospital, treated a case with equally satisfactory results. I attended five cases of heat apoplexy at Barrackpore, and employed this method, and they all recovered.

Now, as to the condition of the patient, and the way in which the remedy acts. Heat, at first, acts as a stimulant on the vaso-motor centres, causing the heart to beat more forcibly and rapidly. But after a long time, the over-stimulated centres become exhausted; then the capillary vessels are dilated fully. This condition is now generally recognised as one of real debility. A writer in the *Lancet* of February 3, 1872, under the head of "Therapeutic Traditions," remarks:—"For the old idea, that sensible heat of skin with redness of the face in itself implies strength of constitution, no authority remains; the obvious fact being that surface redness means *vaso-motor paralysis*." One prominent symptom is noticed in heat apoplexy; that with increased amount of blood in the skin, there is entire suppression of *perspiration*. The sudoriparous glands have apparently lost their power of action. I have an idea that the pathological conditions of heat apoplexy, and the *secondary fever* of cholera are very like one another, each a state of ex-

haustion, the consequence of previous stimulation, and that in both these states stimulants and quinine do good.—*Practitioner*, March 1876, p., 196.

DISEASES OF THE ORGANS OF CIRCULATION.

26.—ON INTERMITTENCY AND IRREGULARITY OF THE PULSE, AND ON PALPITATION, CARDIAC AND AORTIC.

By Dr. GEORGE W. BALFOUR, Edinburgh.

Irregularity of the pulse is most frequently associated with mitral stenosis or with gout; no doubt it is also associated with other cardiac affections, or with apparently simple dyspepsia, rarely, however, I think, unless these affections or that dyspepsia be associated with the gouty dyscrasia. Simple intermittence, however, stands in a somewhat different category; sometimes it is only an early indication of failure of cardiac power dependent upon anæmia, over-work, or worry, or upon valvular disease or gout, but it is often a purely nervous phenomenon. In the former class of cases we have the affection commencing by an intermission, followed by a thump. As the disease progresses this thump becomes associated with a sensation of tumbling, and by-and-by the irregularity and inequality of the heart's action reveals itself to the sufferer by a rapid and irregular succession of thumps and tumbles of varying force. In the class of cases, however, to which I now refer, the disease never progresses beyond the thumping stage, and the thumps are not even very distinct; the patient has at the most an uncomfortable sensation in his cardiac region of varying intensity, lasting for less than a second, and if we happen to feel the pulse at that moment we become aware that this uncomfortable sensation is associated with the omission of a radial pulsation and nothing more; and sometimes this takes place regularly without the patient being in any respect conscious of it. Such cases are of purely nervous origin, and arise from sudden fright, grief, or anxiety; and the intermission thus produced, though for a time of frequent recurrence, gradually dies out, and sometimes disappears entirely, while in many cases it remains permanent, though much more infrequent than at first. This form of intermittence originates in the accidental coincidence of fright or anxiety with a heart congenitally or acquiredly feeble, or with a nervous system from similar causes unduly impressionable. I have known it commence with the shock of a railway accident, and in that case the intermissions were at first every second beat, but in a few months came down to one intermission in twenty beats, and I have no doubt will ultimately disappear. Richardson has known it follow shipwreck in one

instance, with a somewhat similar result, and sudden grief, anxiety, or anger, in several other cases.

This form of intermission is simply an exaggeration of what we have all probably felt at one time or other when our heart has, for the moment, stood still in the face of any impending danger to ourselves or others. It is merely an aggravated form of what our vernacular poet James Smith has so graphically described as indicative of maternal anxiety:—

“My vera heart gaes loup, loup,
Fifty times a day.”

This “loup” being nothing but the perceptible thump which succeeds a momentary intermission. In nervous, and therefore impressionable individuals, now and then, there is no returning thump, the intermission, as Paddy would say, becomes permanent, and we have sudden death from emotional causes, of which not a few are upon record. At other times, partly from the intensity of the impression, but chiefly from debility of the nervous system, this “loup,” thump, or intermission, of which the loup is the most striking subjective symptom, not only occurs under the instantaneous excitement of any emotion, but repeats itself, at first at shorter, afterwards at longer intervals, until at length it dies out under the reassertion of the normal condition of the nervous system. Now and then, however, it never dies out, but repeats itself so long as life continues. We may, however, be permitted to doubt in such cases if the emotional excitement have any other connexion with the intermittence except simply as the accidental incentive to a series of actions already from other causes about to begin. Be that as it may, however, the connexion between emotional excitement and muscular motility of a convulsive and rhythmical character is not an unknown thing in other departments of medicine; and cardiac intermittence and irregular action from emotional causes, finds its counterpart in those imitative choreas and epilepsies, which are of no infrequent occurrence, and has even no very distant connexion with the vagaries of the convulsionnaires and choreomaniacs of the Middle Ages.

In the treatment of intermittent or irregular pulse, we must be guided very much by the condition of the patient, and the existence of any actual cardiac disease, or of any irritation capable of reflexly producing such irregularities. And this we must carefully ascertain for ourselves, and never trust to the mere statements of the patient; because there is nothing more common than for a dyspeptic patient to say, “I never have a headache; I may eat and drink what I please, my stomach never troubles me.” True, but his heart does; and careful examination will discover that his stomach is not so

perfect as he represents it to be. It is precisely the same with him as with a patient with neuralgia of the shoulder-joint and a decayed molar tooth. "You need not look there," he says, "I never have toothache." But he winces when we touch the tooth, and if we get leave to extract it his neuralgia is cured. The one man has toothache in his shoulder, the other dyspepsia in his heart. The cases are analogous, and teach us to put more faith in our own careful examination than in the statements of any patient, which, let me add, however, there is no need to contradict. We are bound to cure our patient if we can, but it would be both thankless and dangerous to attempt to confute all his prejudices.

In the intermittent pulse of infancy and childhood little treatment is required; the bowels must be regulated if necessary, but more by food and exercise than by medicine, for whatever enfeebles the frame tends to keep up the intermittence. We must also by moderate exercise in the open air, early hours, plenty of sleep, and the use of a nutritious but unstimulating diet, seek to tone down any nervous instability, and to develop a state of rude unconscious health. The patient ought also to be warmly clad, and the use of quite cold water as a bath avoided, as any nervous shock ought to be most carefully shunned. If any remedies seem needful, the bromide of iron is a very useful one, or in very irritable patients it may be necessary to have recourse to the bromide of potassium, for a time at least.

In patients affected with cardiac disease, we of course regard the irregular pulse as a mere symptom, and treat the central lesion upon which it depends, whatever that may be. In by far the larger proportion of cases it will be found to be mitral stenosis, as I have already told you; and as the irregular action accompanying this lesion is merely a sign of cardiac debility, what we require to do in these cases is simply to slow and steady the heart's action, increasing at the same time the force of its muscular contractions. All this we can do most effectually by the judicious use of digitalis, so much so, that in a few days the patient will express himself as feeling a new man, and he will not much mind any little remains of irregularity, which it may be difficult if not impossible altogether to remove. Of course, though digitalis in small, repeated, tonic doses, must be our main stand-by in these cases, other drugs as subsidiary agents are frequently of great importance and must be used *pro re nata*; such as carbonate of ammonia when bronchitic rhonchi are present; squill if there be much oedema—if only a little we may safely trust its removal to the digitalis; arsenic if there be much cardiac pain, which there seldom is in these cases; bromide of potassium, with or without morphia—sub-

cutaneously or otherwise—if there be much nervous restlessness; and iron in some form or other if anæmia be a prevalent symptom.

When, however, intermittent or irregular cardiac action comes to be a subject for medical treatment apart from cardiac disease, the heart may still be soothed and steadied by small doses of digitalis if necessary, but the treatment falls mainly under three heads — *First*, to remove the cause if possible; *second*, to brace up the whole organism; and, *third*, to soothe the nervous system. If the cause be mental, such as grief or anxiety, all our care will frequently be baffled, and our success will usually depend not so much upon our remedies as upon our influence, and also upon the amount of mental firmness originally possessed by our patient, and whether he is still capable of being roused to exertion. In nervous shocks from anger or fright, we have a potent help in the narcotic needle, which timeously employed abbreviates the period of shock, and lessens its subsequent influence, besides giving the patient confidence in our resources and in their power to relieve him; the dose of morphia injected must, however, be a full one, and such as is sufficient to ensure sound sleep for some hours. We must in every case caution the patient against all depressing agencies, such as excess in venereal pleasures, excess in tobacco-smoking, too much work, especially intellectual work, all worry or excitement of any kind, and we must prescribe abundance of sleep, fresh country air, plenty of sunlight, perfect quiet, light amusing occupation, and nutritious diet in small quantities at regular intervals, suited to the requirements and capacities of our patient. But no solid food should be given at a less interval than four hours, so as to avoid introducing fresh food into a stomach still containing undigested material, as nothing is more injurious; but a tumblerful of hot water, or a large teacupful of hot solution of Liebig's extract of beef, washes out the stomach, prevents the accumulation of flatulence, and often proves most useful in stimulating the completion of digestion and the emptying of the stomach.

Alcohol is the one domestic remedy which exercises the most potent influence upon an irregular and intermittent heart; it is, however, one which must be used with caution, because excess in its use is apt to perpetuate and increase the very evil it is employed to cure. Still, moderately employed, its action is not only palliative, but to a certain extent curative; only it must be employed in moderate doses, and in those forms which contain fewest substances likely to disagree, and these are in the main good sound claret, and pure whisky free from fusel oil or all injurious impurity. Next to these comes sound sherry, neither too dry nor the reverse, but of medium quality. Porter, ale,

and beer are useful enough at times, as well as all other alcoholic fluids, but as a rule are not to be commended, though in every case we must be guided by the idiosyncrasy, the purse, and the convenience of our patient. The object we seek to attain is to provide a nutritive, diffusible stimulant, slightly narcotic or sedative in its qualities, and one the components of which shall disturb digestion as little as possible, while we also take care that the quantity introduced shall not be sufficient materially to interfere in this way. About two ounces of absolute alcohol is the most that can be introduced into the system in one day without detriment, but this may be given in divided doses, and in various forms, according to the requirements of the patient. Coffee, but especially tea, are excessively injurious to such neurotic patients; they ought therefore to be avoided, and a French breakfast, with meat, fruit, and claret, will be found to suit such a patient much better than our ordinary one with tea for its principal beverage; but if preferred, hot soup with a little well-boiled rice will answer equally well in most cases. For lunch a tumblerful of milk and Carrara water, a glass of beer, or a basin of soup, according to taste. For dinner, plain roast or chop, vegetables in moderation, no pudding, and two or three glasses of claret, or a couple of glasses of sherry, will be found very serviceable. No tea, and at night a glass of whisky and potass water, with or without a biscuit. This is a sort of model diet for such cases, which must be varied to suit each individual case. We must, especially in this climate, secure that whatever is taken in the morning be sufficiently stimulating to enable the patient to withstand the cold—especially in the winter—and to soothe him under the unavoidable worries of life. To this end, if soup be preferred for breakfast, a glass of sherry, or a tablespoonful of brandy stirred into the white of an egg previously dissolved in a little water with a pinch of sugar, will be found a most agreeable and useful lunch. What we must avoid is the production of a catarrhal condition of stomach, or its keeping up, if it already exist, as to that, more perhaps than to anything else, is due the persistence of intermittent cardiac action. But in these cases alcohol is really a remedial agent from which we can obtain more good than from any other drug, only it must be used as a drug, with caution. Definite rules only apply to definite cases, but there are three grand rules which apply to every case, and these are, that the alcohol must be given in a digestible as well as stimulating form, in divided doses throughout the day, and never in excess, otherwise we shall increase the evil we desire to cure.

What we desire to do in such cases is to brace up the general system, at the same time protecting it from injurious influences.

Warm clothing, therefore, is a necessity, and cold sponging of the chest, especially every morning; but bathing, particularly sea-bathing, must be shunned as dangerous, the shock being only too apt to produce, in such cases, spasm of the heart, cramp as it is so often called, which is so instantaneously fatal that the patient sinks, but is not drowned—he is dead before he sinks.

The drugs which will be found most useful vary with each case; pepsine in doses of 10 or 15 grains with each meal seems to do good always, but beyond that we must be guided by subsidiary symptoms. If there be much catarrhal irritation of the stomach, nitric or nitro-hydrochloric acid and calumba or quassia are often useful, soda, potass, or lime in the form of lime-water, often gives great relief, but never produces such a permanent effect as the acids; occasionally the alkaline treatment may be combined with the acid one with advantage, the acids being given before food, the alkalies from half an hour to an hour subsequent to a meal, and with these we may combine the use of podophylline in quarter-grain doses, with a third of a grain of ipecacuan and a quarter of a grain of belladonna, which in enfeebled patients unloads the liver, and relieves the right side of the heart without purging. If torpor of the liver be more marked, then small doses of blue pill and aloes just sufficient gently to move the bowels are most useful. If torpor of the colon be the chief apparent ailment, the long-continued use of Barbadoes aloes in small doses, with sulphate of iron, hyoscyamus, and nux vomica, answers very well; and if much flatulence be present, we may substitute a couple of grains of the compound galbanum pill for the hyoscyamus with advantage; or if gout be the fundamental ailment, then small doses of the acetic extract of colchicum with Barbadoes aloes, both in such doses as shall insure no more than one stool, a little more bulky or looser than usual, and continued daily or every second day for some time, will be found most useful.

When a hæmatinic tonic is required, as will be the case in most instances, the citrate of quinine and iron will be found to be the mildest, and the one most useful in all cases, while Easton's syrup of the phosphates of strychnine, quinine, and iron, is the most powerful, and if continued in drachm doses twice a day for several months, will often effect a most wonderful improvement in the patient's health, and in the state of his heart; that it may do so we must be careful to have all catarrh of the stomach removed in the first place, and the liver also acting freely, otherwise this tonic will not only do no good, but occasionally seems to do harm.

Whenever, from the state of the patient, and the defective secretion of urea, gout seems to be impending, the most im-

portant remedy will be found to be the free administration of colchicum along with alkalies.

Although for temporary purposes there is no sedative equal to the subcutaneous injection of Squire's solution of the bimeconate of morphia, yet for continuous use as a nervine sedative bromide of potassium far surpasses it, but it must be given in full doses, from half a drachm to a drachm three times a day, till its full sedative effect is secured.

You will see then, that for the relief of intermittent and irregular cardiac action, we must endeavour first to determine the lesion upon which it depends, cardiac or otherwise, and we must treat this with due regard to the organic debility to which that lesion owes its injurious efficiency, and we must meanwhile not forget, that between the cause and its effect, we have the nervous system as a connecting link, and that by modifying or interrupting this connection, which we often can do by the judicious use of sedatives and narcotics of various kinds, we may cause to cease, or at all events mitigate the results pending our attempts at cure.

Cardiac palpitation is only too frequently dependent upon similar causes as irregular action, and is to be treated accordingly, especially by such means as shall restore a normal tone to the heart and to the organism generally. Now and then, however, an apparently accidental though violent attack of palpitation seems dependent upon acidity of the stomach, and can often be at once relieved by an antacid draught of soda, potass, or ammonia; and indeed not only palpitation, but also some of the minor forms of irregular action are promptly relieved by a draught containing a drachm of aromatic spirits of ammonia, with or without an equal quantity of tincture of valerian, or failing that, by a tablespoonful of whisky or brandy, with a teaspoonful of carbonate of soda, in about a wineglassful of water, just enough not wholly to drown the miller, as we say in Scotland.

Epigastric pulsation depending on irritability of the abdominal aorta is a local neurosis not always apparently dependent on dyspepsia, nor to be relieved by tonics. I have, however, found it almost invariably to yield to full doses of the bromide of potassium in some bitter infusion such as calumba, gentian, or chiretta. The only exception to this that I remember seeing was that of a woman, in whom this excessive abdominal pulsation was accompanied by a preternatural hardness of that part of the artery, probably due to atheromatous disease, and in her case large doses of the iodide of potassium gave great relief, though nothing had any permanently curative effect.

In connexion with the subject of increased cardiac action generally, I may mention, that while increased action is liable

to follow any unusual exertion, such as climbing a stair or going up a hill, both in hearts valvularly diseased, and also in those which are simply weak, palpitation or irregular action occurring while the patient is at rest is by no means to be regarded as a certain symptom that a heart is only weak or gouty, because of course hearts valvularly diseased are always weak, and often gouty, and therefore liable to present the symptoms of both diseased and also of simply feeble hearts. There is, however, one peculiarity by which the valvularly diseased heart may be perfectly discriminated from a simply weak heart, and that is, that while palpitation or cardiac discomfort occurring as the result of exertion in a heart valvularly diseased can never be relieved by anything but rest, the same results following exertion in the feeble heart of a nervous or gouty individual are frequently calmed down by any emotional excitement, especially of a pleasurable kind, such as meeting a friend, or the sight of anything novel or attractive, or even, strange to say, by a more violent exertion. Thus a man with a heart merely valvularly diseased is not likely to have any discomfort unless he meet with a slight ascent in his walk, when he is at once brought up and must rest; but a man with a gouty or feeble heart, though he too may be "afraid of that which is high," and may also suffer during the ascent, yet has his palpitation at once relieved by any emotional excitement, and if he be seized with sudden palpitation while walking slowly on the level, he will often find it disappear at once if he takes a short race to the next lamp-post: the heart beats the faster for the exertion, but the palpitation is gone, affording an example of a very peculiar form of inhibition, which probably only those can truly appreciate who have experienced it.—*Edinburgh Medical Journal*, Jan. 1876, p. 616.

27.—EXTREME SLOWNESS, FOLLOWED BY GREAT IRREGULARITY OF THE PULSE, WITH NO ORGANIC DISEASE OF HEART, TREATED BY BROMIDE OF POTASSIUM AND BELLADONNA.

By Dr. R. SOMERVILLE and Dr. BRUNTON.

Attention having been called some little time ago in the *British Medical Journal* to cases of extreme rapidity of pulse, I am reminded of a case of extreme slowness, which ought to have been published before this time.

A professional man, aged 37, temperate in his habits, not a smoker, who had enjoyed good, but not robust, health, had for some months been subjected to a great deal of worry in business. For a few days before I saw him he had been out of sorts from irritation of the stomach, but on the night previous to his pulse

becoming so slow, he had gone to bed feeling much better, and in hopes of being able to resume his ordinary duties in the morning. He had most imprudently, however, eaten salt fish to supper. He spent a restless night in consequence, and towards morning vomited several times. I saw him at 9 a.m. By that time he was up and dressed. He made great complaint of a feeling of oppression at the epigastrium. His tongue was clean. His pulse was normal in all respects but in that of rate. Its beats numbered only *twenty-five* in the minute. He was sent back to bed and was ordered a glass of brandy, and to have a mustard poultice applied over the stomach.

11 a.m. Patient no worse, but there is no improvement. To go on with the brandy, taking also beef tea, and some light farinaceous food.

10 p.m. In all respects the same. There has been no vomiting during the day. The rate of the pulse continues the same: it does not count more than twenty-five. The action of the heart, except as regards rate, is normal.

Second day.—Pulse has, during the night, quite changed in character: it is much quicker, and very irregular. The action of the heart is tumultuous. No murmurs to be heard. Patient still complains of great oppression at the præcordium, but there is no vomiting. He is very desponding as to his prospect of recovery. Was seen to-day by Dr. Begbie of Edinburgh, who was of opinion that the disorder of the heart was functional only, and who advised large doses of bromide of potassium with tincture of belladonna.

Third day.—Pulse and heart as yesterday, but præcordial oppression scarcely so great, and patient rather more hopeful.

Fourth day.—Still great irregularity of the pulse, and heart very nearly as tumultuous in action. No vomiting. Bowels acting normally.

Eighth day.—The heart and pulse have, since last entry, continued to improve, and to-day they have attained to perfect regularity of action. Patient is gaining strength, being now able to take substantial nourishment.

It is now more than three years since the above notes were taken, and the gentleman whose case they detail has since then enjoyed good health, and has remained perfectly free from all cardiac symptoms.

[The Editor of the Practitioner, Dr. BRUNTON, makes the following very interesting comments on this curious case.]

There can be little or no doubt that the peculiar symptoms observed in this case were due to irritation of the pneumogastric nerve. It was long ago discovered by Edward Weber that this nerve, when irritated, either stopped the heart altogether or

greatly lessened the frequency of its pulsations in animals, and in 1865, Czermak showed that it possessed the same power in man. This physiologist was able to irritate his own vagus nerve on the right side by pressing with the finger at the inner side of the sterno-mastoid muscle, and thus squeezing the nerve against a small, hard lump, which I have heard, from good authority, was really an exostosis from one of the cervical vertebræ, although it is often said to have been an enlarged and hardened gland. Whenever he did this, his pulse fell to less than half its normal rate, and he experienced a peculiar sensation of oppression or constriction in the sternal region. As these were exactly the symptoms presented by the case just narrated, I think we may fairly conclude that both the slow pulse and the epigastric oppression observed in it were caused by irritation of the vagus, and may proceed to consider what part of the nerve was affected. The nerve may be divided into three parts, viz.: its roots in the medulla oblongata, its trunk, and its terminal branches in the heart. The effect upon the pulse will be much the same whatever part of the nerve is irritated. The medulla is the part in which alterations in the activity of the nerve, leading to changes in the pulse rate, usually take place. The trunk is only affected in rare instances, such as that of Czermak; but the terminal branches in the heart are more frequently acted upon by poisons such as nicotine, conveyed to them by the blood. These poisons may at the same time exercise a similar action on the medulla. In the present instance there was nothing whatever to indicate that the trunk of the vagus was affected, and the point of irritation might be either the roots in the medulla or the ends in the heart. Now, the supper of salt fish was evidently the cause of the disturbance, but it is not very easy to say whether it produced its effects by acting as a poison or simply by irritating the stomach. There are a good many instances on record of a subtle animal poison becoming developed in salt fish or sausages; a poison which not only eludes the efforts of the chemist to detect it, but which seems to have a curiously limited action, frequently doing no harm to the lower animals, though proving fatal to man. Although the symptoms observed in this instance differed considerably from those usually produced by the poison generated in salt fish, it is nevertheless possible that this was a case of the sort, and that the peculiarities it presented were due to the poison being in very small quantity, but acting on a subject rendered exceedingly susceptible by previous overwork. If present at all, the poison must have been in such small quantity as not to affect persons in ordinary health, for the others who partook of the salt fish at the same time experienced no inconvenience from it. But it is also possible that the fish produced

slowness of the heart simply by irritating the stomach, and thus causing reflex irritation of the vagus and slowing of the cardiac pulsations, as Meyer and Pribram found pinching the stomach to do. If such were the case, the irritation would be conducted by the afferent nerves from the stomach to the medulla, and thence downwards by the vagus to the heart.

It is, as I have already said, exceedingly difficult to decide whether the salt fish acted in one or other, or both, of the ways just mentioned. In most cases of poisoning by salt fish, the pulse is either normal or quicker than usual, and this militates against the supposition that the slowness in the present instance was due to a special poison generated in the fish; and, on the other hand, although the slowness continued a considerable time after vomiting had ceased, there was probably still some gastric disturbance present, as it was not until the eighth day, when the pulse had become almost quite regular, that the patient was able to take substantial nourishment. It seems, therefore, not improbable that the affection of the heart was due to reflected irritation from the stomach, and, although the symptoms were so different, might be classed along with those cases in which we find increased pulsation and palpitation depending on gastric disorder.

It is, however, also possible that a special poison might have something to do with it, and the treatment adopted was such as to meet both possibilities.

Supposing it to be of a reflex nature, the bromide of potassium would be useful, as it seems to have the property of diminishing the activity of reflex centres generally, and by acting in this way upon the medulla, it would prevent the irritation it had received from the stomach from being transmitted down the vagus to the heart.

If, however, there was a special poison in the blood, irritating either the roots of the vagus in the medulla or its ends in the heart, the bromide would be of little use, but belladonna would be successful. For belladonna and its alkaloid, atropia, in large doses, paralyse those nervous structures of the heart in which the vagus ends, so that no irritation of any part of the nerve whatever can slow the pulsations. As doses of belladonna, large enough to paralyse the vagus, usually produce dryness of the mouth, delirium, and other unpleasant symptoms, the drug was not pushed to its full extent in the present instance, trust being rather placed in the bromide; although, had the interference with the heart's action been such as to threaten danger to life, recourse might have been had with advantage to large doses of belladonna.—*Practitioner*, March 1876, p. 186.

DISEASES OF THE ORGANS OF RESPIRATION.

28.—ON THE COMBINED USE OF MORPHIA AND ATROPIA
IN THE TREATMENT OF SPASMODIC ASTHMA.

By Dr. G. OLIVER.

[The writer corroborates the opinion of Dr. Keith Anderson as to the value of morphia given hypodermically in the treatment of spasmodic asthma, during the paroxysm.]

While regarding morphia as the principal agent in relieving asthmatic spasm, I have found the combination of it with atropia to act more quickly, and to combat the attacks more completely and effectually. Atropia seems to assist morphia not only in quelling the nervo-muscular storm, but in obviating the distressing and depressing after-symptoms of morphia narcosis which now and then are met with. Besides this, belladonna is now fairly established in the therapeutics of asthma chiefly through the advocacy of Trousseau, Seé, Hyde Salter, and G. Wood.

We may look upon the subcutaneous use of the active principle — atropia — as of value at least equal to that of the ordinary administration of belladonna.

There is still another reason why atropia may enable the asthmatic to derive more benefit and a safer relief from the combination of it with morphia than from morphia alone. Dr. Seé has shown (Practitioner, 1869) that belladonna quickens respiration, and, during the paroxysm, the altered respiratory rhythm of asthmatic breathing is changed to one of a different type; but, during the second stage, “the breathing becomes slower from fatigue of the pneumogastrics, and we have now a renewed indication for the use of belladonna, which will quicken respiration again. It is for this reason that you must not prescribe opiates, as is often done by physicians who are called in during the attack, and will insist on soothing the patient at any cost. Opium acts inversely to belladonna; it diminishes the frequency of breathing to the damage of the patient.” While the pneumogastrics are exhausted, may not the injurious depression of morphia be checked by the stimulating power of atropia? Here the guarded use of opiates is a cardinal point of treatment; but to secure from them a relief from suffering without dangerous depression is a real gain.

But an appeal to experience is all-important:—

In July, 1871, a lady, aged 30, came under my care suffering intensely from spasmodic asthma of a somewhat bronchitic type, yet the neurotic element was very prominent. She had suffered for four years from very violent paroxysms, usually ushered in by slight bronchitis: each attack confining her to

bed for a week or two, and the quiet intervals were too short to allow her to regain her strength properly. The lungs and heart were free from disease. Most of the ordinary remedies had been tried with only partial relief. The late Dr. Salter prescribed for her large doses of belladonna with ipecacuanha, which, affording somewhat more benefit than other drugs, were freely consumed in desperation by the patient who was hungering for relief at any cost. With dilated pupils, dry throat and foul tongue, the agonising spasm would only partially relax, and an unusually severe midnight paroxysm, which I was summoned to treat, would not give way at all. I injected a third of a grain of acetate of morphia;—in about ten minutes the breathing was calm and nearly free from wheezing. On awaking next morning nausea, vomiting, and headache, detracted somewhat from the joy of my patient at having discovered a remedy so effectual and so speedy in its operation. After repeating the injection with the same results, I combined with the morphia $\frac{1}{100}$ gr. of sulphate of atropia. Within a shorter period—not exceeding five minutes—the breathing was comfortable and free from wheezing; and next morning nausea, vomiting, &c., were not complained of. On one or two occasions I used atropia alone; the result being relief to the breathing in about ten minutes. It was quite clear the relief was much less decided than after morphia alone, and this was inferior to the combination of both remedies which set the patient's breathing perfectly at ease for the night without the penalty of vomiting and headache in the morning. The subsequent history of this case has shown these points of experience:—

1. Morphia and atropia are superior to morphia alone; the good effect is more speedy and complete, and they produce no depressing gastric disturbance.

2. During the first year of subcutaneous treatment the asthmatic paroxysms were, as a rule, met by the injections as soon as relief was urgently demanded. The patient struggled on with her attacks before calling in her medical attendant who, only then, injected the remedies. While these attacks in mid-career were always quickly brought under control, they—as severe asthmatic paroxysms—notwithstanding the continued use of the injections, required time for complete and safe subsidence. While thankful for a means so effectual as this in quelling the violent storms, my patient sought for further benefit by attacking the earliest approach of them by the injections. This combined prophylactic and curative use of the remedies has been resorted to during the last three years, and, during this long period, there has not been one severe attack as in former years. At the very onset of bronchial dis-

turbance, an injection night and morning for a few days together has usually kept the threatened asthmatic spasm in abeyance and apparently stamped out the beginnings of an attack of the old type. Besides this, the patient asserts there is less susceptibility to bronchial attacks as well as the accompanying asthma, and the intervals between the attacks are lengthened, asthmatic breathing being often totally absent for a month or six weeks.

3. The very frequent use of the injections does not appear to injure the general health.

4. Speedy relief. The effect is generally declared in five minutes in the form of comfortable sleep and quiet breathing. The most intense attack has given way completely in from fifteen to twenty minutes.

This case shows that we may arrest in the earliest stage an attack of spasmodic asthma, thus preventing much suffering, removing the evil effects of the attack, and prolonging the intervals of ease. The nerve-bias, fostered by every attack, which ingrain more and more the "asthmatic habit," may be persistently and successfully resisted by the repeated subcutaneous employment of morphia and atropia. — *Practitioner*, Feb. 1876, p. 137.

29.—HOW TO CURE A COLD IN THE HEAD.

By Dr. DAVID FERRIER, Assistant Physician to King's College Hospital.

[We all know the miseries of a cold in the head, and the inconvenience arising from it. Dr. Ferrier having succeeded in arresting one with which he was threatened, by the treatment recommended, brings it under the notice of the profession.]

The symptoms being those of acute catarrh of the nasal mucous membrane, the treatment which seemed to me most likely to succeed was that which I have always found most efficacious in acute catarrh of the gastric mucous membranes. In the acute catarrh of alcoholism accompanied with profuse secretion of mucus, which is often vomited up in large quantities almost without effort, as well as in the more chronic forms of gastric catarrh, bismuth alone, or in combination with morphia, acts almost like a specific.

On the same principle the topical application of bismuth to the nasal mucous membrane seemed to me the plan most likely to be followed by beneficial results. I do not know whether the plan is absolutely original, but I am not aware of its having been adopted previously. This, however, is of no importance compared with the question of its efficacy.

On the evening in question I began to suffer with the symptoms of cold in the head—irritation of the nostrils, sneezing, watering of the eyes, and commencing flow of the mucous secretion. Having some trisnitrate of bismuth at hand, I took repeated pinches of it in the form of snuff, inhaling it strongly, so as to carry it well into the interior of the nostrils. In a short time the tickling in the nostrils and sneezing ceased, next morning all traces of coryza had completely disappeared.

Bismuth alone, therefore, proved quite successful, but it is better in combination with the ingredients in the following formula. Bismuth by itself is rather heavy, and not easily inhaled, and it is, moreover, necessary that it should form a coating on the mucous membrane. It is, therefore, advisable to combine it with pulv. acaciæ, which renders the bulk larger and the powder more easily inhaled, while the secretion of the nostrils causes the formation of an adherent mucilaginous coating, of itself a great sedative of an irritated surface. The sedative effect is greatly strengthened by the addition of a small quantity of hydrochlorate of morphia, which speedily allays the feeling of irritation, and aids in putting a stop to the reflex secretion of tears.

The formula which I find on the whole the most suitable combination of the ingredients of the snuff is as follows:—Hydrochlorate of morphia, two grains; acacia powder, two drachms; trisnitrate of bismuth, six drachms. As this is neither an errhine nor a sternutatory, but rather the opposite, it may be termed an anti-errhine or anti-sternutatory powder. Of this powder one-quarter to one-half may be taken as snuff in the course of the twenty-four hours. The inhalations ought to be commenced as soon as the symptoms of coryza begin to show themselves, and should be used frequently at first, so as to keep the interior of the nostrils constantly well coated. Each time the nostrils are cleared another pinch should be taken. It may be taken in the ordinary manner from between the thumb and fore-finger, but a much more efficacious and less wasteful method is to use a small gutter of paper, or a “snuff-spoon,” placing it just within the nostril and sniffing up forcibly so as to carry it well within. Some of the snuff usually finds its way into the pharynx, and acts as a good topical application there, should there be also pharyngeal catarrh. The powder causes scarcely any perceptible sensation. A slight smarting may occur if the mucous membrane is much irritated and inflamed, but it rapidly disappears. After a few sniffs of the powder, a perceptible amelioration of the symptoms ensues, and in the course of a few hours, the powder being inhaled from time to time, all the symptoms may have entirely disappeared.

I am writing this note cured of a cold in the head which I began to manifest in a very decided manner last night—viz., weight in the frontal sinuses, tickling of the nostrils, sneezing, watering of the eyes, and commencing flow of the nasal mucus.

I commenced taking the snuff, continuing at intervals for about two hours, thoroughly coating the interior of the nostrils with it. Next morning I found myself entirely free from catarrh. The effects in my own case have been twice so rapid and beneficial that I look with comparative indifference on future colds. In the case of others to whom I have recommended the same treatment equally rapid and beneficial results have followed. One of my students in King's College Hospital described the effects as quite magical and unexpected, having in this way got rid of a cold in one evening. The other day one of the officials in King's College asked me if I could do anything to check a dreadful cold in the head which he had just caught. I gave him the above prescription, asking him to note the results. A day or two after he came and told me that I had given him very marvellous snuff, as he had not taken more than one-eighth part before he had got rid of all his uneasiness and discomfort. Though I have not yet had very many opportunities of trying this method of cure, the success so far has been such as to warrant my recommending it as a rapid and efficacious treatment of nasal catarrh.—*Lancet*, April 8, 1876, p. 525.

[We should prefer to use this remedy without the morphia.—EDS.]

DISEASES OF THE URINARY ORGANS.

30.—PATHOLOGY AND TREATMENT OF ALBUMINURIA.

By Dr. T. LAUDER BRUNTON, F.R.S., &c., Editor of the *Practitioner*.

[Dr. Brunton has given us a very interesting paper on this subject. How many cases we have of anæmia—the patients being blanched, and overcome by the least exertion.]

The blood has to act as a carrier of oxygen from the lungs to the tissues. All muscular work, all secretion from glands, in fact we may say, every process of life is a process of combustion. Whenever the supply of oxygen fails the work is lessened, and at length stopped entirely, just as the exclusion of air would extinguish the fire in a furnace, and bring to a complete standstill the engine to which it furnishes the motor power.

Now the combustion in the body goes on at a great distance from the outer air, and it would stop for want of a sufficient

supply of oxygen were it not for the hæmoglobin of the blood. The hæmoglobin is the red colouring matter of the blood, and is contained in the red corpuscles alone. When they are deficient it is deficient, and its chief function and theirs is to act as a carrier of oxygen. It takes up this gas in the lungs, carries it to the tissues, and then gives it up to them. At each breath that a man takes he saturates a quantity of the hæmoglobin in the bloodvessels of his lungs with oxygen, and then on it goes to his muscles to keep up combustion in them. If he is running, leaping, rowing, or using his muscles much in any way whatever, they must get more oxygen than when he is sitting or lying, and in order to give them as much as they need he must breathe much oftener than usual. By doing this, a healthy man can get enough oxygen to enable him to make great exertions, and if his work is only moderately hard he scarcely feels that his breathing is different from usual. But if he unfortunately happens to have a quantity of fluid in his chest, so that one lung can only expand to half the size it ought to do, he cannot take in the proper quantity of oxygen at each respiration. He is therefore obliged to breathe oftener so as to make up for the small quantity of air that he can take in each time, and so a moderate exertion will make him puff and blow like a healthy man after a hard run.

Now, exactly the same thing takes place when a person has too little hæmoglobin and red blood corpuscles as when he has too little available lung tissue. It is easy to see this. If a man has only half the quantity of hæmoglobin in his blood that he ought to have, it can only take up about half the amount of oxygen that it ought to do, and there is not much to choose between this condition and the case where only one lung is working, and thus giving only half the proper quantity of oxygen to the blood. If the blood only *takes* half the right quantity of oxygen, it is much the same as if it only *gets* half the right quantity.

Thus it is that anæmic persons are short of breath, and if they go quickly up stairs, they puff and blow so that they cannot speak when they get to the top, and, in short, they are incapable of any great exertion.

Thus the first symptom of anæmia, not to mention the pallor, is muscular weakness; the second is shortness of breath.

The third symptom, or set of symptoms, refers to secretion. As I have already said, secretion is a process of oxidation as much and perhaps even more than muscular contraction is. Ludwig and Spiess found that the saliva secreted by the sub-maxillary gland of a dog was actually $1\frac{1}{2}^{\circ}$ C. warmer than the blood of the carotid artery, and of course to heat the saliva in this way there must be very active combustion going on in the

gland during the process of secretion. To supply the oxygen required for this combustion, the arteries of the glands are found to dilate, and torrents of bright-red oxygenated blood rush rapidly through them. Nor is this process confined to the salivary glands alone. The stomach, the pancreas, and probably also the intestines, all get a copious supply of arterial blood during the process of secretion, and although a rise of temperature indicative of active combustion has not been demonstrated to take place in them during the act of secretion, it is in the highest degree probable that these other glands resemble the salivary ones in this respect. Seeing, then, that oxidation plays such an important part in secretion, we could hardly expect that it would go on briskly, any more than the muscles would work easily when the blood is poor in hæmoglobin, and the supply of oxygen which it ought to convey to the glands is consequently deficient.

Now the whole process of digestion depends on secretion, and if the salivary glands and pancreas do not secrete properly, the starchy part of the food will not be rendered soluble; if the stomach does not pour out gastric juice, the albumen and fibrin will not be digested; and if the pancreas is not working, the fats will pass out unchanged. And even when these organs do their work after a fashion and succeed in digesting the food at length, yet if they secrete their appropriate juices in small quantity the food will be only slowly digested. It will then undergo partial decomposition in the intestine instead of being rapidly dissolved and will give rise to the formation of gas in the intestines with all its attendant discomforts. The symptoms of this condition are—there is no appetite, the food lies heavy at the stomach, and there is frequent belching of gas which often brings up with it particles of undigested food or sour burning liquids. There is often nausea, sometimes retching, or vomiting. The bowels are sometimes very loose and at others very confined.

We may thus add to muscular weakness and shortness of breath a third symptom of anæmia, viz. *dyspepsia*.

Having thus found that the muscular, respiratory, and digestive functions are all impaired by anæmia, we naturally inquire how the nervous system gets along. The brain cannot work without oxygen any more than the muscles, indeed it requires a still more constant supply than they, and when we direct our attention to it we find that it enjoys no exemption from the general debility. Deficiency of motor power manifests itself in languor, listlessness, and laziness, while the sensory functions evidence their disturbance by giddiness, drowsiness, headache, weight in the head, throbbing of the arteries, neuralgic affections, and numberless odd nervous symptoms, which lead medi-

cal men who are themselves suffering from anæmia to believe that they have all sorts of mischief going on within their brain-pan, until a little quassia and iron removes their symptoms and allays their apprehensions.

We come lastly to the circulation, and we find that it too suffers, so that although we have put it last, one of the symptoms connected with it sometimes attracts our attention more than all the others together. This symptom is œdema or swelling from the presence of fluid in the cellular tissues.

How does this fluid come to be there? During life there is a constant exudation of fluid from the bloodvessels, and after this has done its work by affording nutriment to the cells of which these tissues are composed, it is again absorbed, partly by the veins and partly by the lymphatics. In health the exudation and absorption balance each other and so there is no accumulation of fluid in the tissues; but whenever the exudation becomes too rapid, or the absorption becomes too slow, such an accumulation takes place. When it occurs in the tissues it is called œdema; when in a large lymphatic space, better known perhaps by its more common name of serous cavity, it is termed dropsy.

Now absorption has been shown by Goltz to depend very much on the vaso-motor nerves. When these are acting powerfully, absorption takes place rapidly; when they are acting slightly, absorption takes place slowly.

Deficient action of these nerves also causes fluid to be poured out more rapidly from the vessels, as well as to be absorbed more slowly. Thus any weakness of the vaso-motor system tends to produce œdema in a double fashion, and to such weakness we may probably ascribe the œdema we meet with in anæmia. It is quite evident that if the œdema is dependent on deficient re-absorption, we shall find it most marked in those parts where the circulation in the veins and lymphatics is most languid—*i.e.*, in the feet and ankles; and this is indeed what we do find in œdema depending on venous obstruction; but when it depends on excessive exudation also, it may be found wherever the tissues are loose, as in the lower eyelid and scrotum. In health the vaso-motor nerves keep the arterioles throughout the body more or less contracted, and thus prevent the blood which the heart pumps into the aorta from running out quickly into the veins. They thus keep the arteries always well filled and fairly on the stretch, so that the heart cannot empty itself into them, and thus stretch them still more without using a fair amount of force. But when the vaso-motor system gets weak, it no longer keeps the arterioles up to their work, and in consequence they occasionally dilate and let the blood through them more quickly than they ought. The

arteries consequently get partially emptied; the heart can pump all the blood into them with almost no exertion, and so it sometimes seems as if it did not know what to do with its superabundant energy, and works fast and loose like the driving-wheel of a locomotive on a piece of greasy rail. Another symptom of anæmia is, therefore, palpitation.

We may now at last count up on our fingers the symptoms of anæmia, not reckoning, but always bearing in mind, the pallor of the patient. We have (1) muscular weakness; (2) shortness of breath; (3) dyspepsia; (4) weakened nervous system, both sensory and motor; (5) weak circulation leading to palpitation, and œdema or dropsy.

All the varied symptoms included under these five heads may, as we have seen, be caused by anæmia, though we must not forget that some of them may be present without anæmia. For example, an individual may come complaining of dyspepsia, though a single glance at his plethoric countenance shows you that no want of blood is the cause, but rather gourmandizing and gluttony above the powers of even the most healthy stomach to sustain. But supposing we have all the symptoms included under the five heads just given, and the pale face and blanched mucous membranes leave no doubt on our minds as to the presence of anæmia, we have next to ask what is the cause of it, in order that, instead of trying to remedy one symptom by itself and another symptom by itself, we may strike at the root of the disease and remove the anæmia on which they all depend.

There are two ways in which a man may come to want money; (1) he may not get enough to supply his daily wants, either from laziness or misfortune; or (2) he may lose what he has either by being extravagant and squandering until poverty compels him to tighten his purse-strings—steadily living above his income, and thus losing his fortune by degrees; or by rashly speculating, and thus losing a great part of it at one stroke.

And just so is it with the blood. We use up some of it every day to keep our tissues in working order; and unless this loss were regularly compensated by the food we eat our blood would soon be too much reduced in quality, if not in quantity, to sustain life. For be it remembered that a proper quantity of blood will not nourish the tissues if its quality be deficient, any more than an ounce of copper will pay a man's debts when they amount to an ounce of gold. Whenever, then, this nourishment, which is daily received into the blood, is insufficient to supply the daily expenditure on the tissues, anæmia will, in a greater or less degree, be sure to follow. It does not very much matter whether the supply of food taken into the mouth

is insufficient, or whether imperfect digestion prevents assimilation. Dyspepsia, then, by lessening the appetite, and rendering the digestion of such food as is consumed less perfect than it ought to be, has much the same effect as partial starvation and is one cause of anæmia.

But anæmia we have already seen is a cause of dyspepsia; and thus the one aiding the other they go on from bad to worse, in a vicious circle unless something interferes to break it.

The cases of anæmia which I would liken to the spendthrift are those of chlorotic girls where the anæmia really seems to depend on some of the organs or tissues using up the blood in a way they have no right to do, and thus squandering the precious fluid without getting any good from it, and indeed without one's being able to see where it has gone, unless the deep-red colour of the urine one gets in such cases on the addition of nitric acid to it may be taken as an indication that more than a fair share of the hæmoglobin of the blood has undergone some mysterious transformation, and found too ready a way of escape through the kidneys.

Anæmia may also be a consequence of sudden and considerable losses of blood, such as those which occur in menorrhagia, or from bleeding piles, which not unfrequently drain away the vital fluid and sap the patient's strength, ere ever he be aware of the mischief which is going on.

Important as these causes of anæmia are, there is yet another no less important, and in which the drain is still more constant, although in it one may not see the blood actually leave the body while still retaining its well-known colour.

This cause to which I wish especially to direct your attention is albuminuria.

In it the blood is constantly losing one of its most important constituents, the albumen, which leaks through the kidneys along with the urine, and thus runs to waste instead of remaining in the body and ministering to its nutriment. Although the serum-albumen thus lost is not a constituent of the red blood corpuscles yet the deficiency in it seems in some way to affect them, and their numbers diminish possibly, though we cannot say with certainty, because they cannot get the albuminous constituents of their hæmoglobin in requisite quantity. For it must be remembered that the hæmoglobin, or colouring matter of the blood which plays such an important part in the oxygenation of the tissues, consists of an albuminous substance, globulin, and a non-albuminous substance, hæmatin. There are regulating arrangements in the body which keep all its different parts in mutual harmony and dependence on one another, so that, as St. Paul said long ago, "if one member suffer, all the members suffer with it," and if one part is

starved, the other parts frequently, if not always, suffer privation with it. The serum-albumen being, then, deficient in the blood, it is not at all astonishing that the albuminous constituent of the colouring matter should also be below par.

The symptoms of albuminuria are, therefore, those of anæmia, and we often suspect the presence of the disease from the mere look of the patient before we have addressed a question to him or applied a single instrument of physical diagnosis. There is not only paleness from the general want of blood, but there is a greater tendency to œdema than in other forms of anæmia, so that the face is not only pale, but puffy, pasty, or doughy-looking, with a tendency to swelling about the lower eyelids. The ankles and shins are frequently œdematous and pit on pressure, and there may also be accumulations of fluid in one or other of the serous cavities. These appearances in a patient at once arouse a suspicion of albuminuria, and we proceed to test them by examining the urine. In a state of health this secretion should be absolutely free from albumen. We detect the presence of this abnormal constituent in two ways: 1st, By boiling; and, 2nd, by adding nitric acid. On boiling urine containing albumen, coagulation takes place, and according to the quantity present we have either a faint haze giving an opalescence to the liquid, a heavy coagulum, or any intermediate condition between these two. There are some fallacies in this test, however, which require attention, for ignorance of them may lead us to imagine that there is no albumen when it is really present, or to fancy it there when the urine is completely free from it. The first fallacy is that serum-albumen forms compounds both with alkalis and acids, to which the names of alkali-albumen and acid-albumen have been given respectively. Now neither of these compounds are coagulated by heat, and although serum-albumen itself is readily coagulated by boiling, yet if acids or alkalis are present when we begin to heat it, the very warmth of the liquid, as we gradually raise its temperature, causes the albumen to combine with the acids or alkalis and form acid-albumen, or alkali-albumen. Thus it may happen that by the time we reach the temperature at which the albumen should be precipitated it is no longer present in its original condition, its combinations being already complete.

Thus, if the urine be very strongly acid, or very strongly alkaline, we may get these compounds formed, and then heat produces no coagulation, although albumen is present. We therefore ascertain the reaction of the urine by means of litmus paper before applying heat. As the tissues of the body are all alkaline, it is very unlikely that the urine will contain so much free acid as to produce acid-albumen, and indeed such a condi-

tion is almost never found except in persons who have been taking large quantities of mineral acids. In such a case we would add sufficient alkali merely to neutralise the acid before we boiled the urine, but this is so rarely necessary that we generally disregard this source of error. Excessive alkalinity, however, is not so uncommon, and we very frequently have to add a few drops of acetic acid to the urine so as to render it slightly acid before boiling.

This addition of acid, however, serves a double purpose, and not only enables us to detect the albumen more certainly when it is present, but prevents us from mistaking other things for it when it is absent.

In some urines alkaline phosphates or carbonates are precipitated by heat and may be mistaken for albumen, but a drop or two of acetic or nitric acid prevents their precipitation, or if added after they have already been thrown down dissolves them again and causes the urine which they have clouded to become clear again.

It is thus evident that heat alone without acid is an insufficient test for albumen, and nitric acid alone without heat is also unsatisfactory and may be deceptive; but nitric acid coagulates albumen and causes a haze or coagulum just like heat. If the urine contain a large quantity of urea, the nitric acid may cause the formation of a crystalline precipitate of nitrate of urea, which is, however, but rarely mistaken for albumen. But if urates be present in large proportion, one is much more likely to fall into error, for the nitric acid drives out uric acid from its combinations with soda or potash, and free uric acid being much less soluble than urates a precipitate is formed which is much more likely than the urea one to be mistaken for albumen. A little heat now applied to the urine causes either urea or uric acid to redissolve, and the urine clouded by them to clear; but it has no effect on the haze or coagulum produced by albumen.

In testing for albumen, then, the best method of proceeding is to allow three or four drops of nitric acid to trickle down the side of the test tube containing the urine. If no haze appears we may conclude that the urine is free from albumen. It won't do to pour in acid until the test tube is half full, for it is possible that if only a little albumen is present it may be converted into acid-albumen and dissolved by the concentrated acid. If a cloudiness appears we must not at once conclude that it is due to albumen, but must warm the urine over a spirit lamp. If it is really albuminous the opacity will remain, but if the cloud is due to urea or uric acid it will disappear.

So much for the symptoms of albuminuria, which are paleness and pastiness of the face, a tendency to œdema and dropsy,

complaints of weakness, shortness of breath, dyspepsia, nervous symptoms, and, I may add, occasionally palpitation.—*Practitioner*, Nov. 1875, p. 352.

31.—GALLIC ACID IN THE TREATMENT OF ALBUMINURIA.

By Dr. J. T. JAMIESON.

The following is from an article in the *American Practitioner*:—I wish to call attention to the use of gallic acid in the treatment of albuminuria as a sequel to scarlet fever, with which, in a few cases, I have met with marked success. My experience with the remedy has been as follows:—

In my first case, occurring in a boy aged about twelve years, the symptoms were very severe. There was oedema of the face and lower extremities, but no effusion into the thoracic or abdominal cavities; violent headache; blindness; there had been four or five strong epileptiform convulsions; urine was scanty and contained blood, resembling exactly water in which fresh beef had been washed, and coagulating about one-half on testing with heat and nitric acid. To relieve the cerebral symptoms, a blister was applied to the neck, sinapisms to the extremities and lumbar region, cold to the head, and two or three doses of a mercurial with bitartrate of potassa. This was followed by iodide of potassium and a teaspoonful of a saturated solution of gallic acid every two hours. The acid was given in this manner for five days and nights in succession, the patient rapidly improving under its use, and the urine becoming more copious and less bloody. It was continued for twenty-two days, only at longer intervals, and at that date the urine when tested manifested the slightest possible trace of albumen, although the boy at this time was around the house and apparently perfectly recovered, having been so for a number of days. The tinct. ferri chloridi was given in small doses, and completed the cure.

My second case occurred in a girl about six years of age. The eruption was very livid and the skin had desquamated. The child recovered well from the fever, and was about the house. She went into a cold room to play with other children, and a day or two after the face became oedematous; there was pain in the head; slight fever; urine quite bloody, and on testing in the usual manner presented considerable coagulation. The patient was put upon a saturated solution of gallic acid, a teaspoonful every two hours. In seven days the urine was free from albumen and copious in quantity, and the child seemed well, with the exception of debility, for which the muriated tincture of iron was prescribed. About ten days after this, in consequence of fresh exposure to cold, there was a slight relapse, the urine

becoming again bloody and the face puffed; but on resuming the gallic acid for a few days these symptoms speedily subsided and the recovery became permanent. In this case the gallic acid was administered unaccompanied by any other medicine, except an occasional dose of castor-oil to regulate the action of the bowels.

Remarks.—The treatment hitherto generally adopted in this affection has been that of acting derivatively on the bowels by means of mercurials, followed by such diuretics as digitalis, sweet spirits of nitre, acetate of potash, &c.; but if future experience should confirm the efficiency of gallic acid, I cannot but think we shall possess a remedy superior to any of the above. The gallic acid if I understand its action aright, enters the blood unchanged, and unchanged is carried directly to the congested and inflamed capillaries of the secernent portion of the kidneys, acting as an astringent and tonic upon them, promoting their contraction, and thus arresting the exudation of red blood corpuscles and promoting the normal secretion of urine. I have seen no unpleasant effects from its administration as freely as above represented. It does not disturb the stomach nor interfere with the appetite or digestion, but it does tend to constipate the bowels somewhat, rendering necessary the occasional use of a relaxative.—*Medical Press and Circular*, Feb. 2, 1876, p. 104.

32.—ON A CASE OF DIABETES CURED BY THE SKIM-MILK DIET.

By Dr. SCOTT DONKIN.

At a meeting of the Clinical Society of London, held on December 10th last, Dr. Scott Donkin brought forward a case of diabetes cured by the use of skim-milk. The patient, who was shown to the Society, was a hale, robust-looking man, forty-five years of age, a carman by trade. He had an accident three years ago, suffering a blow on the head, which laid him up for awhile. At the end of June, 1875, he had another accident: his cart coming into collision with a cab, he fell between the shafts, and was dragged some distance and bruised, but not severely injured. Soon afterwards he began to suffer from polyuria, with great debility and much thirst, but the practitioner whom he consulted did not recognise the case as one of diabetes, and sent him into the country for change of air. There, on an unrestricted diet, he grew rapidly worse, and, as the existence of diabetes was suspected, he was sent back to London. On October 6th he was first seen by Dr. Donkin, having then suffered from these symptoms for six weeks. At that time he was passing daily from twelve to four-

teen pints of urine, of sp. gr. 1045, containing a quantity of sugar, which was estimated, by means of Duboscq's polarising saccharometer, at twenty-eight grains to the ounce. There was urgent thirst, a parched mouth, dry skin, and great voracity, and the sight was impaired. There was loss of sleep from the frequent calls to micturate at night, great debility, and loss of flesh, but no other complications. The patient was at once (on Oct. 7th) put on skim-milk diet, seven pints a day, without other food. Two days later the quantity of milk was increased to eight pints a day. The treatment was followed by a rapid diminution in the quantity of urine, and on the fifth day from its commencement only four pints and a half of urine were passed. On October 14th, or seven days from the commencement, the specific gravity of the urine was 1010, and there was no sugar to be discovered in it. The health had greatly improved, the patient slept well, having no calls to pass urine at night. Nine pints of milk were now allowed per diem, a part being given as curd. On November 5th three-quarters of a pound of mutton-chop was added to the diet, the skim-milk being reduced to six pints. On the 12th the specific gravity of the urine was 1012, and he was allowed an additional chop. On November 18th, or six weeks from the commencement of the treatment, the patient's health was restored and there was no sugar in the urine, and he was allowed some fish. On the 21st he resumed his occupation, which he has since continued. On December 3rd he was seen again; he came then free from disease. On the 10th his urine was of sp. gr. 1017; no sugar could be discovered with Fehling's solution, and the quantity was from four pints to four pints and a half per diem. He was now allowed some tea and a little pale brandy. In remarking on the case, Dr. Donkin observed that some might be inclined to regard it as one of traumatic origin, but this was not his opinion. The great quantity of sugar passed and the rapid emaciation showed the case to be a very severe one. It was a fact of great importance that the copious formation of sugar was arrested under a diet which contained eight ounces of milk sugar per diem, that being the estimated quantity in the amount of milk taken, and it corroborated the observation which he had previously made, that lactose does not cause an increase in diabetic sugar. Dr. Donkin also gave some particulars of another case under his care—a young lady ten years of age, who for three months had been suffering from great thirst and frequent micturition. The practitioner who was consulted found the case to be one of diabetes. The specific gravity of the urine was 1040, and it contained much sugar. He ordered a meat diet and small quantities of Dover's powder. She still, however, suffered

from thirst and occasional giddiness, and on October 2nd she was put on a diet of skim-milk, which she continued for ten days. This was followed by a rapid fall in the specific gravity of the urine, which reached 1010 on October 12th, when she came to London to consult Dr. Donkin. The same treatment was continued, and on November 6th she was seen by Sir T. Watson, who stated that the specific gravity of the urine was 1015, and it contained neither sugar nor albumen. Since that time the patient had continued well, and on December 6th the specific gravity of the urine, which had varied from 1014 to 1016, was 1023 (the quantity of urine being less than usual); the child was in good health and spirits, and the diet was being gradually altered towards an ordinary one. Dr. Donkin remarked that these cases show in a striking way the arrest of the disease by the treatment when it is applied in the early stages, before the disease has been fully established or has produced irremediable lesions. The fact that the method was so often applied only as a last resource, when all other measures have failed, must be borne in mind in estimating the value of the treatment; this was especially the case with hospital patients, who so often only apply for relief when broken down and worn out by the disease, and hence the frequent failure to obtain any successful result.—Dr. Coupland enquired whether the patient was of temperate habits, or was in the habit of taking malt liquors.—Dr. Donkin replied that he was very temperate and moderate.—Dr. Glover suggested that the urine should be examined by the secretaries in order that confirmation might be given to the remarkable facts of the case. One very striking fact in the history of the case was that the patient had actually, as he had ascertained by questioning him, lived solely on skim-milk for a month. Moreover, he was restored to perfect health. Now if any other remedy were introduced which produced half so remarkable effects, we should think it a very important addition to the pharmacopœia. The second case was even more astonishing, as the disease in old people may often be held in check for a long time by careful dieting, but in early life they were usually much less amenable to treatment.—At the suggestion of the chairman, Dr. Glover and Dr. Southey made an examination of the urine, and reported that it was entirely free from sugar.—*Lancet*, Dec. 18, 1875, p. 879.

SURGERY.

FRACTURES, DISLOCATIONS, AMPUTATIONS, & DISEASES
OF BONES, JOINTS, ETC.

33.—ON EXCISION OF THE ANKLE.

By JAMES F. WEST, Esq., Senior Surgeon to the Queen's
Hospital, Birmingham.

[Excision of the ankle has not hitherto been regarded with much favour for chronic disease of the ankle-joint.]

Resection of the ankle, or, in fact, of any other joint, is of course not justifiable until attempts have been made to remedy the joint-mischief by all the means at our disposal. Rest; immobilisation; counter-irritation, by blisters, iodine, or by the actual cautery; and, lastly, free incision into the joint and removal, if possible, of any fragments of loose necrosed bone,—must each, in succession, have been resorted to; while at the same time the patient's general condition has been to the utmost improved by fresh air, good living, and such tonics as iron, quinine, and cod-liver oil.

Still, after the adoption of all these measures, suppuration in and about the joint, pulpy degeneration of synovial membrane, ulceration of cartilage, and caries of the cancellous structure of the bones, will in some cases continue, and then arises the question, Shall the leg be amputated by Syme's or Pirogoff's method, or shall the patient have the chance first offered to him of keeping his foot, even although this will necessitate a somewhat difficult and tedious operation and a protracted convalescence?

I entertain a strong opinion that by adopting Ollier's (of Lyons) mode of subperiosteal resection, the future history of excision of the ankle will be far more satisfactory than the past has been. Experience has also shown that partial resections had better be given up, and that it is better to excise the entire joint than to limit the operation to the removal of the obviously diseased portions of it. Attention to the following measures during the after-treatment will also add greatly to the number of successful cases:—1. Early and complete im-

mobilisation of the limb. 2. The use of the anterior iron bracketed splint, by means of which, as after resections of the knee, the leg can be suspended in a Salter's swing, and the lateral openings formed in the joint dressed without much disturbance of the parts. Lastly, the employment of drainage-tubes, passed from one side of the ankle to the other, by which pus and carious débris may be removed, and through which the wound may be daily washed out by some weak disinfecting solution, as carbolic acid or Condy's fluid.

The originator of resections, as of so many things that still hold good in surgery, was Celsus, as we learn from this passage: "Ideo quod excedit, abscindendum est." But the first to apply this principle to the ankle was an English provincial surgeon, Hey, of Leeds, who in 1766 resected the lower ends of the tibia and fibula for compound dislocation; and since that time resection of the ankle has frequently been performed under similar circumstances with such good results that few would question the propriety of performing resection for compound dislocation of that joint, rather than of at once recommending amputation of the leg. Hey's success led to the adoption of the operation for chronic articular disease by Moreau, and in 1792 he first removed the lower ends of tibia and fibula for disease.

[Many surgeons have written about and performed this operation, but perhaps no paper is of so much value as that by Mr. Hancock (*Retrospect*, July to Dec., 1867, p. 185.)]

In this paper Mr. Hancock referred to 19 successful cases of his own, and mentioned that Heyfelder had given 26 examples of this operation, of which 5 died, 1 suffered consecutive amputation, and the rest proved successful; and that of Jaeger's 24 cases 23 proved successful and only 1 died.

[A lad, sixteen years of age, injured his ankle by a jump from a height of ten feet. Chronic disease of the joint resulted.]

The lad's general health being good, and the case seeming favourable for resection, the operation was performed as follows:—Lateral incisions were made parallel to the tibia and fibula; the fibula was first divided with the chain saw, then the tibia with Butcher's saw, and lastly the astragalus with Hey's saw. The articular surfaces of all the bones were found to be devoid of cartilage and superficially necrosed. The operation was performed by Ollier's subperiosteal method, and, Esmarch's bandage having been used, it was quite bloodless. No tendons were divided. A drainage-tube was inserted between the bones of the leg and foot, and the limb at once immobilised with bandages and a gutta-percha splint. Chloroform was used, and well borne by the patient.

[In Dec. 1875 this lad was able to walk a distance of four or five miles without inconvenience.]

My manner of making the incisions in this case was almost identical with that described and recommended by Hancock, except that the lateral incisions were not carried forwards and downwards on the tibial side as far as the projection of the inner cuneiform bone, nor on the fibular side to within half an inch of the base of the outer metatarsal bone; such free incisions are, in my opinion, rarely necessary, and further tend rather to lead to subsequent suppuration among the tarsal bones. Lateral incisions are far preferable to the semicircular incisions which I have seen employed at the Leeds Infirmary and elsewhere, inasmuch as with the former all the tendons are saved, and no vessel of importance is divided. Excision of the ankle is one of those operations in which the advantages afforded by Esmarch's bandage are most appreciated. Not only is the patient free from hemorrhage, but in consequence of the parts not being covered with blood, we can see clearly what we are doing, and need only take away as much of either of the bones as is absolutely diseased, and if on taking one section of bone caries is still evident, we can take slice after slice of bone away until we get to sound osseous tissue, without having our view of the joint cavity in any way obstructed. The joint having been opened on the outer side and the fibula exposed, I separate the periosteum from it with the raspatory, and then cut off the lower end of that bone with the chain-saw. I prefer the chain-saw because it can readily be passed round the bone, and can be kept close to it, so as not to endanger the peronei tendons, and hence I think it presents greater advantages than Hey's or Butcher's saw. I then divide the internal lateral ligament, keeping close to the bone, so as to avoid the flexor tendons and the posterior tibial artery, push the lower end of the tibia through the fibular opening, denude the periosteum, and cut the articular surface and inner malleolus off with Butcher's saw, and lastly with Hey's saw I remove the upper part of the astragalus. Should that bone be extensively diseased it should be freely gouged, and the carious or necrosed portions cleared out with the osteotrite. In some cases it has been successfully removed entire, along with some portion of other of the tarsal bones, as in Mr. Swain's case of a boy, aged fourteen, for whom at various times he excised the ankle-joint and the bones of the tarsus for caries; but, as a rule, when more than one of the tarsal bones is largely affected with caries, it will be better to amputate at once by Syme's method. Pirogoff's operation, though recommended by some surgeons, is not, as I have before proved, suitable in such cases, in consequence of the great risk of caries recurring in the remaining portion of the os calcis.

I hold that complete resection of the joint is better than partial, and in that respect I differ from Mr. Bryant, who says, "Resection in cases of disease is very hazardous, except in partial resection of the fibula, which is generally successful." Now, having myself tried partial resection of the fibula more than once, and especially recently on a boy aged sixteen, whose disease seemed limited to that bone, and having had subsequently to submit the patient to secondary amputation, I cannot agree with Mr. Bryant's opinion, and I think he will modify it when he has had some personal experience of complete resection of the ankle, which up to the present time he admits he has not had.

Immobilisation of the limb before the patient leaves the operating-table I deem to be essential. I care very little whether that condition be obtained by a gutta-percha or leather case or by a starch or plaster of Paris bandage with suitable windows on either side of the ankle, but the immobilisation must be thorough, and the apparatus, whatever it is, should not, if it can be avoided, be disturbed for at least a couple of months. The limb may, with comfort to the patient, be suspended by one of Bartleet's or Watson's anterior splints in a Salter's swing.

As new bone is often formed rapidly in young subjects, and concomitantly there is an abundance of exudation material about the ankle, it is advisable in the course of two or three months to give support and compression to the new tissues by means of a piece of sheet lead one-eighth of an inch in thickness, retained *in situ* by careful bandaging.

Another feature of the operation of great value, to my thinking, is the preservation of the periosteum by means of careful employment of the raspatory, so as to avoid slitting that membrane. The instrument should be kept as close to the bone as possible, so as to detach some of the bone granules along with the periosteum.

The idea of regenerating bone by this means, so generally attributed to Ollier, of Lyons, belongs, as Giraldès candidly points out in his "Mal. Chir. des Enfants," to our countryman Goodsir, who, as early as 1845, discovered the fact that by preservation of the periosteum with the subjacent and still adherent fine particles of bone new bone could be formed.

Preservation of the length of the limb and greater consolidation of the parts, both points of immense importance as regards the patient's future condition, are gained by this method of subperiosteal resection, and I cordially echo all that has been said in its favour by Professor von Langenbeck in his work "On the After-results of Joint Resections," and not less by another great authority in the treatment of diseases of joints,

Dr. Sayre, of New York. Drainage-tubes applied at the time of resection, so that the joint cavity may be kept free from discharge, and thoroughly cleansed daily with some weak disinfectant solution, are small, though not unimportant, matters to be attended to in the after-treatment of the case.—*Lancet*, Dec. 18, 1875, p. 868.

34.—DEMONSTRATIONS OF ANTISEPTIC SURGERY, BEFORE MEMBERS OF THE BRITISH MEDICAL ASSOCIATION IN THE OPERATING THEATRE OF THE ROYAL INFIRMARY, AUGUST, 1875.

By JOSEPH LISTER, Esq., F.R.S., Professor of Clinical Surgery in the University of Edinburgh.

[The first case was one of aneurism of the femoral artery, and was an illustration of the effects of ligature of an artery in its continuity by means of prepared catgut applied antiseptically.]

The operation was performed on the 23rd of June, and the wound was absolutely skin-whole in fifteen days, without the occurrence of any suppuration at all. [Mr. Furneaux Jordan, of Birmingham, was now kind enough to come forward and examine the patient, verifying the fact that there was no pulsation in the artery at the groin. Mr. Lister then proceeded.]

The immediate object of the operation has therefore been attained—the vessel has been permanently obstructed at the part tied; and this has been done without the occurrence of any suppuration, and by a mode which, I think, we may venture to say, involves no danger whatever, provided it be properly carried out. The two great risks of an operation like this are, of course, secondary hemorrhage, and diffuse suppuration in the cellular tissue around the peritoneum; and both of these are securely guarded against by proceeding in this manner. I believe myself that this is a pretty perfect method of obstructing a vessel in its continuity; I do not see that we can wish to have it improved upon. I therefore regret extremely to find that it is still distrusted in various quarters, even by those who use catgut for the ligature of arteries in ordinary wounds. They do not trust it for tying arterial trunks in their continuity. I regret this the more, because I feel it is to a certain extent my own fault. When I first published on the subject, I was not aware myself of the proper mode of preparing the catgut. I had prepared it right, but by a mere accident. I described the mode of preparation in the *Lancet* (April 3, 1869), as steeping the catgut in a mixture of carbolic acid and oil. It so happened that the carbolic acid which I used was liquid carbolic acid, so called—that is to say, crystallized car-

bolic acid, liquefied by the addition of water. Now, this water makes all the difference in the world. When oil is added to this liquid carbolic acid, a considerable portion of the water is deposited in the form of very fine particles, which are suspended in the oil; and it is this mixture—this emulsion, if we may so call it—of oil and water which causes the remarkable physical change in the animal tissue of which catgut is composed, that alone renders it fit for our objects. The tissue of the catgut in the ordinary condition is utterly unfit for surgical purposes; as slippery, when moistened, as a piece of intestine in the dead-house—when you tie it in a knot, it slips with the utmost ease. But after it has been steeping in the emulsion of carbolic acid, water, and oil, for a certain length of time, it undergoes a physical change, which I am quite at a loss to explain. As the tissue lies steeping in this mixture, the first effect is to moisten it somewhat; then, as time passes, after about a week, you find that, instead of becoming softer, more swollen and more opaque, as you would expect, it is, on the contrary, growing less opaque and beginning to shrink; and in about three months, though still softer than dry catgut, it is comparatively firm, and quite transparent. Now, if you take a fresh piece of dry catgut and put it into this same sample of the preparing liquid, you will find the second piece become in the first instance partially moistened like the first; a fact which renders it inexplicable to me, why the former piece should have undergone what looks like a partial drying. But whatever the explanation, the all-important fact is this, that after the catgut has been thus partially dried, so to speak, in this moist liquid, it is now no longer liable to be made slippery by being steeped in water or the animal juices at the temperature of the body: it is indeed rendered softer and somewhat opalescent, but a reef-knot tied upon it holds better than one on waxed silk. I repeat, when I first published on the subject, I was not aware of this circumstance. I had got the catgut properly prepared, but it was by mere accident that the water which is essential to the process was present in the mixture that I used; and, ignorant of its importance, I omitted to mention it in the description which I gave of the mode of preparation; whereas mere steeping of catgut in a solution of dry carbolic acid in oil, though it of course makes it antiseptic, leaves it perfectly unfit for use as regards its physical properties. When I found out my mistake, I sought to remedy it by insisting, in subsequent publications, upon the importance of the presence of the water in the preparation of the catgut; but I never stated, as I now do, that I had originally described an untrustworthy method. I very much regret this bad result of what turns out to have been premature publication; and I

earnestly hope that this public confession of my mistake will have the effect of preventing any further bad consequences from it.

The catgut does not spoil by being kept a long time in the preparing fluid of oil, carbolic acid, and water. Here is some that was put in six years ago last month. It is now just as good as ever. Thin as it is, I cannot break it with any reasonable force. If you were going to tie the external iliac, you would use a thicker piece than this; partly, in order that it may stand any strain to which it could be reasonably subjected in the act of ligature, and partly that, a longer time being required for the absorption of the more substantial material, it may remain longer as a mechanical barrier to the force of the circulation.

In this point of view there is another important advantage possessed by catgut properly prepared, viz., that it is much less rapidly absorbed than that which has been for a shorter time in the preparing liquid.

I would strongly advise any surgeon, who proposes to ligature an artery in its continuity with catgut, to test for himself the quality of the article; since those who sell it are tempted, if their stock of old catgut has run out, to supply that which has not been long enough prepared. In order to ascertain if it is trustworthy, a piece should be steeped for an hour in water about the temperature of the body, as in a vessel at a suitable distance from the fire. If then a reef-knot tied upon it does not slip, it is fit for use. And it will be well for the surgeon to keep a stock of the material for special purposes like these, testing it in the first instance in the manner described, after which he will be sure that, being still kept in the preparing liquid, it will be at least equally good at any subsequent period. If these points are attended to, there will be no further complaints about untrustworthiness of the catgut.

When it is requisite that the cord should be able to withstand all the strain to which the human hands can subject it, as, for example, if it be used for the pedicle in ovariectomy (in which case, I may remark, the pedicle would have to be well subdivided), the material must be of specially strong quality to begin with. Catgut consists of the peritoneum, together with muscular fibres, of the small intestine of the sheep; and the common kinds are either the entire tube of the gut, or longitudinal strips (according to the thickness required) simply twisted, dried, and subjected to sulphurous acid vapour, or other chemical agents. But for special purposes, as, for example, the manufacture of fiddle-strings, the cord is made of several narrow strips twisted together, and is then very much stronger. Such catgut can be obtained of the musical-instru-

ment makers, but is of course then unprepared in our sense, unfit for surgical purposes, and must be kept in the preparing liquid for a due length of time. For the sake of those who wish to prepare catgut for themselves, I may repeat here the proportions which I have found the best for the purpose. Add one measure of water to ten parts by weight of crystallized carbolic acid, mix and add one measure of the mixture to five measures of olive-oil, in a suitable jar or wide-mouthed bottle; then at once introduce the catgut, the hanks being opened up to allow access of the liquid to them; cover, and set aside in a cool place. Some water is gradually precipitated to the bottom of the vessel, and it is necessary to prevent any part of the gut from coming in contact with this precipitated water. A simple way of ensuring this is to put in as many marbles as will cover the bottom of the vessel.—*Edinburgh Med. Journal*, Dec. 18, 1875, p. 485.

35.—ON PROF. LISTER'S TREATMENT OF WOUNDS AND ABSCESSSES BY THE ANTISEPTIC METHOD.

By THOMAS SMITH, Esq., Surgeon to St. Bartholomew's Hospital.

[Both Mr. Smith and his house surgeon, Mr. Vernon, have studied antiseptic surgery under Prof. Lister's and Mr. Annandale's personal instructions.]

The theory, or, one may now say, the facts on which Mr. Lister's antiseptic treatment rests are as follows:—

1st. That in the dust of the atmosphere, and on matter with which it is in contact, there are the germs of minute organisms which, under favourable circumstances, induce putrefaction in fluids and solids capable of that change, in the same manner as the yeast-plant occasions the alcoholic fermentation in a saccharine solution.

2nd. That putrefaction is not occasioned by the chemical action of oxygen or any other gas, but by the fermentative agency of these organisms.

3rd. That the vitality or potency of the germs can be destroyed by heat, and by various chemical substances, which we call, in surgery, "antiseptics."

Now, I am not going to ask you to believe these statements on my authority, but I will shortly refer to the results of experiments performed by Pasteur, Lister, Sanderson, Tyndall, and others, which justify the above conclusions.

It is scarcely necessary to state that organic fluids, like milk, urine, and blood, infusion of meat, &c., if kept in contact with the air at ordinary temperatures, will ere long decompose or putrefy, and will give evidence of putrefaction by turbidity (if

the fluid be originally clear), by the evolution of offensive gases, and by the development within them of bacteria.

Again, I need do no more than remind you that prolonged boiling will not of itself preserve such fluids from putrefaction. Yet any of these or similar fluids may be kept free from putrefaction for an indefinite time, in spite of free access of the atmospheric gases, provided that the fluid has been boiled at the outset to destroy any organisms in it, and that the vessel containing it has been thoroughly purified by heat, and that the dust of the air is excluded. The exclusion of the dust may be effected in various ways. In some of Pasteur's first experiments it was done by having the neck of the flask which contained the liquid drawn out by aid of heat into a fine tube bent at various angles, in which form, though open at the end, and allowing perpetual entrance and exit of air, it arrested all particles suspended in it, and the urine or other fluid which was the subject of experiment remained permanently unaltered. Or, again, the same object may be attained by having the mouth of the flask plugged with a mass of purified cotton-wool, which effectually filters of its dust the air that enters the vessel in consequence of the condensation which alternates with expansion in the diurnal changes of temperature. But if the neck of the flask is broken short in Pasteur's experiment, or the plug of cotton-wool removed, organisms are sure to show themselves before many days have passed. Even more striking is the method adopted by Mr. Lister, who decants the boiled organic liquids into wine-glasses purified by heat, and each covered with a glass cap similarly purified and a glass shade, scrupulous care being taken to avoid the entrance of dust during the process of decanting. Neither cap nor shade fits closely, so that a constant interchange takes place between the external air and that in the wine-glass, yet the double protection afforded by the cap and shade effectually excludes dust, and the result is, that although the organic liquids gradually diminish in bulk by evaporation, and in the course of months dry up altogether, no organisms make their appearance from first to last, nor does putrefaction or any other fermentative change occur. If, however, the glass shade and cap are removed for a few minutes and replaced, fungi or bacteria soon show themselves. But he has found that if the glass cap be only lifted for a second or two in an ordinary apartment free from draughts there is practically no risk of the entrance of any organism in the short period of exposure.

Further, it has been shown by Pasteur and other observers that it is by no means essential to the success of such experiments that the organic liquids should be boiled, but that when circumstances admit of their being withdrawn uncontaminated

from their natural receptacles, such as the urinary bladder, the bloodvessels, the udder of the cow, or the shell of a fresh-laid egg, they will remain free from organisms and from putrefaction when kept in pure vessels and protected from dust.

It has also been discovered that impure air will purify itself by mere subsidence of its dust. Pasteur long ago proved that putrescible fluids could be kept free from putrefaction in air taken from cellars free from draughts, when the solid particles of the atmosphere had had time to deposit themselves by subsidence; and Prof. Tyndall has recently subjected air purified by being kept at rest to very searching tests to ascertain if it will excite putrefaction in putrescible solutions. He has found that solutions of meat, cheese, turnip, &c., first subjected to a high temperature, can be kept free from putrefaction for an indefinite time exposed to the air-closed boxes that have been kept at rest a day or two, to allow the dust to subside, precautions being taken to prevent the said dust rising again by coating the inside of the box with glycerine. The same experimenter has demonstrated the fact that the air which has been thus rendered incapable of exciting putrefaction — *i.e.*, aseptic — is also optically pure: that is, that there are no particles or motes to be detected in it when illuminated by a beam of electric light in a darkened room.

I think, then, we are justified in concluding that in the dust of the atmosphere there *are* such things as fermentative particles, organisms, germs, or whatever you like to call them, and that these, under favourable circumstances, induce putrefaction in fluids and solids capable of the process; that without these germs putrefaction and the formation of bacteria does not take place, and, finally, that these germs can be destroyed or removed from the atmosphere by the various means that I have above described.

Let me here remark, as having an important bearing upon Mr. Lister's practice, that in the case of those fluids that have been kept free from putrefaction by any of the above described means, the addition of the smallest drop of ordinary water, or the contact of a glass rod that has not been specially treated to render it aseptic, will almost certainly excite putrefaction, though all other prescribed conditions are scrupulously carried out to prevent its occurrence.

On the other hand, Mr. Lister has found that when any portion of apparatus used in investigations on this subject cannot conveniently be purified by heat, the object may be attained by washing the glass or other material with a strong watery solution of carbolic acid, and drying it with a carbolised rag, and in the course of a long series of experiments he has invariably found this antiseptic agent as efficacious as the flame of a spirit-

lamp in preventing the growth of organisms and the occurrence of putrefaction.

Mr. Lister's object in the treatment of wounds and abscesses is to exclude from them these germs of organisms that float in the atmosphere and are the causes of putrefaction, and the means he employs for effecting this purpose he recommends, not as the best that can be used, but as the best that he has been able up to the present time to devise; and although Mr. Lister considers the truth of his theory incontrovertible, yet he does not claim to have brought his practice to perfection.

Mr. Lister claims for his plan that when it can be carried out with due care and proper observance of details, he can, as a rule, secure that an open wound should heal after the manner of a subcutaneous injury—that is, without inflammation or constitutional fever, and for the most part without suppuration; while, if suppuration occurs, he secures that it shall not be putrefactive—that is, accompanied by the changes that we consider evidences of putrefaction, such as the formation of bacteria and the evolution of fetid gases.

In the treatment of abscesses by the antiseptic method, Mr. Lister believes that he has effected an entire revolution in the course of the disease after the cavity has been opened. I may here mention that, along with many local advantages, the patient is said to be free from all danger of irritative fever as the immediate consequence, and from hectic at a later stage.—*Lancet*, March 25, 1876, p. 453.

36.—ON THE ANTISEPTIC TREATMENT OF CASES OF OPEN KNEE-JOINT.

By Dr. HECTOR C. CAMERON, Extra-Mural Lecturer on Surgery, and Surgeon to the Royal Infirmary, Glasgow.

The recent discussions on antiseptic surgery have revealed a very general desire that the subject should receive further clinical illustration, and it is this consideration which encourages me to offer my experience in the matter. In the present paper are given the details of the cases of open knee-joint which I have treated by this means. The mode of procedure adopted in all of these was essentially the same, with the exception that, in the first case, no drainage-tube was used. The dressing was, on each occasion, conducted from its commencement to its close under a spray of carbolic acid. The joint having been frequently and thoroughly injected with a watery solution of the acid (1 to 20), an antiseptic drainage-tube was introduced into the wound. Over this was placed a small piece of antiseptic gauze which had been wetted in a solution of the acid (1 to 40), and over that, again, one or two

dry loose pieces of the same material. Outside all was fixed, by an antiseptic gauze bandage, a large square dressing, consisting of eight layers of antiseptic gauze, under the top layer of which was interposed a piece of thin macintosh.

Case 1.—Case of open knee-joint; primary amputation of shoulder-joint; incised wound of face; traumatic delirium; recovery.—Wm. B., aged thirty-two, was admitted into Ward 23 of the Glasgow Royal Infirmary, of which I was in temporary charge, on Aug. 23rd, 1873, having been knocked down by a passing train, as he was walking along a line of rails sixteen miles from Glasgow. On admission he was in a boisterous state of intoxication.

The right arm had been severed from the trunk. About three inches of the humerus remained, the greater part of it being bare and stripped of covering. The soft parts had been torn away internally as high as the apex of the axilla, and externally from a little below the point of the acromion process. There was an incised wound about an inch and a half in length beneath the left eye, exposing the malar bone. Over the right knee-joint, a little below and to the outer side of the patella, was a lacerated wound about two inches and a half in length. My finger, introduced into that wound, passed readily into the knee-joint, and it also discovered the fact that there was considerable undermining of the soft parts, downwards towards the leg and backwards into the popliteal space.

The patient having been put under chloroform, the head of the humerus was disarticulated and a quantity of torn muscle cut away. The principal vessels, which had been tied before admission with silk ligatures, were ligatured higher up with antiseptic catgut and the vessels cut off below. All the intricacies of the wound were now carefully and thoroughly washed with a watery solution of carbolic acid (1 to 20), and the usual dressings applied under a spray of the acid. A large raw surface was left from the deficiency of the soft parts.

The open knee-joint was next dealt with. Under the spray it was injected freely and repeatedly with a watery solution of the acid (1 to 20). Two antiseptic stitches were introduced and the usual dressings applied. A piece of Gooch's splint was placed at the posterior aspect of the limb, extending from the upper part of the thigh to below the middle of the leg. The wound over the malar bone was injected, stitched and dressed antiseptically.

During the night the patient proved very restless and tore off the dressings from the shoulder and face. They were soon reapplied by the house-surgeon, but were again removed by the patient. In the morning (Aug. 24th) both wounds were exposed. In his struggles he had also broken the splint

placed at the back of the knee into two pieces, but the dressings were not seriously disturbed. His pulse was 96, and his temperature $100\cdot2^{\circ}$. Antiseptic dressings were not reapplied to the shoulder and face, as they had been off nearly the whole of the night. The knee was dressed under the spray. There was found to be considerable effusion in the knee-joint (I had used no drainage-tube) and under the undermined tissues. Little or no blood or other fluid had escaped. I therefore removed the stitches and introduced my finger, previously carefully washed in the solution, into the wound, giving exit to a considerable quantity of bloody serous fluid. Dressings applied as before, and a new splint placed at the back of the limb. Pulse, in the evening, 114; temperature $102\cdot4^{\circ}$. On Aug. 25th the discharge from the shoulder had a distinctly putrefactive odour, and it was ordered to be washed and dressed with Condyl's fluid and water, changed frequently. Occasionally he removed the dressings, at other times he allowed them to remain. The knee had furnished a considerable amount of bloody oozing, but there was no redness, and only slight swelling. The wound had gaped and appeared to be draining freely. He was still delirious and now full of hallucinations, although not so boisterous as he had been. For the next three days his mental condition showed typically the usual characters of traumatic delirium. The shoulder soon began to suppurate freely. The wound over the malar bone, which had been left quite bare, healed by first intention. The knee, at first dressed daily, was found to furnish only some serous oozing, and to be free from redness, pain, and swelling. The pulse continued quick, but of good strength. Two abscesses formed in the neighbourhood of the shoulder, from bagging of the discharge, and were incised. The suppuration from the wound and these incisions was abundant, and in spite of the frequent changing of the dressings by the nurse (three times a day), had a distinctly offensive odour. After the fifth day the serous discharge from the knee was found so slight that it was dressed once or twice on alternate days only, and latterly every third day. About the fifth day also the delirium ceased, and he began to sleep better and to take more food. During the first five days he broke the splint on four separate occasions, and always exactly opposite the popliteal space. By the middle of September the wound of the knee was clearly quite superficial, and in a very short time soundly cicatrised. *From first to last it furnished no pus whatever.* The wound of the shoulder, which had been further increased by some sloughing of the integument, discharged freely for a considerable time; and its neighbourhood continued red, swollen, and tender to the touch. Ultimately the discharge diminished, and the large wound contracted and

cicatrised, so that, when he was dismissed on Oct. 6th, it was quite superficial. He left the hospital at his own desire, but was still very weak. He ultimately got strong and well, the right knee-joint being perfectly flexible.

Remarks.—This case is an interesting one, not only as a recovery after severe and complicated injury, but as an example of three very different methods of healing going on side by side in the same individual. In such a case all must admit that the special behaviour of each separate wound is due to local and not to constitutional causes. The wound of the face healed by first intention; that of the shoulder by granulation and suppuration, with sloughing of bruised and lacerated tissue, and the formation of abscess in the immediate neighbourhood. The wound of the knee healed by neither the one plan nor the other. And yet this is a distinction which writers and debaters on this subject are not always sufficiently candid to draw. It is a too frequent reply to one who narrates the successful issue by antiseptic treatment of an open knee-joint, such as that just related, that an equally successful result is often obtained without any antiseptic dressings at all. And no doubt this is perfectly true. In a joint which has been purposely incised (as for the removal of a loose cartilage), or into which an accidental wound has been made by some such sharp-cutting instrument as a scythe, it is *possible* that a favourable result may be got by any carefully-applied dressing. Union by the first intention may occur there, just as it did in my patient's cheek. But should the wound of the knee-joint be allowed to gape, or should it be, as it too commonly is, of such a character as renders union by first intention impossible, there is no alternative except that it shall putrefy, granulate and suppurate, and that the patient shall run the usual well-recognised risk of the loss of his limb, or his life, or both. In the above case matters were altogether different. In spite of severe complication, this open joint, although not entirely healed until after the lapse of three weeks, from first to last behaved like a subcutaneous injury, and externally furnished only a little clear, watery discharge. It may be viewed as the severest, in the first instance, of the three injuries from which the patient suffered; and yet it was the wound of the shoulder which caused him all his pain and occasioned the whole of our anxiety. To the shoulder, moreover, was directed all the "fussiness" which we displayed in the treatment of the case, for, while it was dressed three times a day, the dressings of the knee were changed, during the greater part of the time, only once in three days. Indeed, to an onlooker, unaware of the advantages of antiseptic treatment, this disregard of the knee wound would have been inexplicable, and might have been deemed culpable. And

yet many would have it to appear that the success of the antiseptic method of treatment is entirely due to what they are pleased to call the extra care and attention bestowed on the wound. Lastly, I would remark that this case illustrates also the fact, with which I am very familiar, that a non-putrefying wound, so far from involving an extra expenditure of time, trouble, and dressing materials, is, other things being equal, economical in respect of all of them.—*Lancet*, Jan. 22, 1876, p. 123.

37.—ON NON-UNION OF FRACTURES.

By H. O. THOMAS, Esq., Liverpool.

So many varied methods have been practised for the relief of ununited fractures that the addition of another seems almost superfluous. The majority of the methods previously in use were attended with more or less risk in their application, and were all but impossible where the fractures were very close to or involved a joint; as for instance the non-union of a fractured condyle of femur, the head of the tibia involving the knee articulation, or tibia and fibula in the ankle. A case of non-union of the latter character came under my care in March, 1874.

Case 1.—The patient, Mr. John T., of Southport, was admitted into my hospital on the 16th of March, suffering from fracture of lower end of fibula and tibia. In this case there was so much abnormal motion between the fractured bones, permitting outward luxation of the foot, that the internal malleolus of the tibia nearly came in contact with the ground when the patient placed the weight of his body on the limb, and the fracture threatened to become compound. The deformity was easily reduced, but recurred with the slightest weight of the body to this extreme degree. As none of the usual methods could be applied in this case with any safety, I decided to try what effect a repeated application of percussion, with a rubber-protected mallet applied all round the articulation, would have. I accordingly commenced the treatment on the 20th of March, and applied percussion every second or third day for four weeks, desisting only for a few days when the parts became irritable. There was applied no splint or appliance whatever. From the first day of the operation there was a daily increase of consolidation and stiffness of the part, which continued until it resulted in a complete bony union in the space of four weeks; and on the 24th of May the patient was dismissed from the hospital quite cured, and free from this defect, the lower end of the tibia and fibula being con-

siderably enlarged. In this case the non-union had existed for a period of twelve months.

This was the first instance in which I tried this method of treatment, but, finding it to answer beyond all my expectations, I decided to adopt it in future cases so as to give it a more extended trial.

Case 2.—On the 19th of March last I was consulted by one Harry B., of 30, Gradwell-street. This patient was suffering from a fractured leg, with non-union after sixty-five days' treatment. Assisted by my friend Dr. William Kelly the patient was placed under æther, and the tibia percussed carefully for ten minutes, the skin being protected during the operation with a layer of felt. The percussion was in this case followed by a considerable amount of swelling and irritation. The limb was placed in a suitable appliance, and at the expiration of four weeks complete consolidation took place. In this case percussion was only once performed.

Case 3.—On the 14th of May last I was consulted by John McA., who was suffering from a fractured leg sustained on the 23rd of February, 1874, on board the steamer "Caspian." He had been in the hospital at Halifax for eight weeks, and on his arrival in England, suffered from non-union, and consulted a specialist in this town, under whose care he remained for five months. At the end of that period he was for eight weeks a patient of one of the charities, and on the 14th of May last became an indoor patient in my hospital, when I performed percussion upon the tibia with the effect of consolidating the fracture in six weeks. This was percussed only on one occasion.

Case 4.—On the 1st of January last I was consulted by James K., a coloured man, who had his humerus fractured while steering one of the Dominion steamers in a storm. The patient was two weeks at sea before receiving any professional assistance. On his arrival at this port he consulted me, and the fracture was fixed in my usual appliance and continued in that condition for two months with only partial union. I noticed that while the union seemed complete on making a lateral strain, it seemed to be quite loose if strained antero-posteriorly, imitating a hinge-joint. On the 28th of April I commenced to percuss the humerus, protecting the skin, and did so for four days, until there was a decided enlargement about the fracture. A rapid consolidation took place, which was complete in a month's time.

Case 5.—On the 15th of July last I was consulted by John McN., who fractured his leg on the 13th of October, 1874. He was for ten weeks an indoor patient of a charity, and for fifteen weeks more an outdoor patient. I found on examina-

tion that there was non-union, and decided upon operating. Having administered æther, the limb of the patient was well percussed with a copper mallet, rubber-faced. During the operation the patient partially recovered from the æther, and getting possession of the mallet, was almost successful in applying it to the writer and his assistants. We succeeded, however, in disarming him, administered more æther, and finished the operation. The operation not being followed by much periosteal irritation, I repeated it every week for six weeks, but not under æther. The limb was retained in a suitable appliance for maintaining immobility, and on the 1st of September the limb was quite strong and well consolidated.

Case 6.—John Dark N., on the 15th of June last, fell into ship's hold and fractured his thigh, and was admitted into a charity for treatment, where he remained seven weeks, at the expiration of this period became an outdoor patient for three weeks, and, judging that the limb was becoming weaker, consulted me, when I found, on examination, non-union of the femur. I operated by my percussive method for a period of five minutes, producing a considerable degree of irritation. On the first day, and on every fourth day for three occasions, repeated the application in a milder form. During this time and up to the occurrence of consolidation he was well fixed in a suitable appliance with a satisfactory result.

Case 7.—In January last I was consulted by Peter G., who, while rescuing a fellow-workman from some machinery, had himself become entangled and suffered a compound fracture of the forearm (radius, and ulna). The limb was carefully placed in a suitable appliance for a compound fracture, and was not disturbed for three months, when, on examination, I found that the radius had not united. I continued the treatment for two months longer, when, finding that the radius was still ununited, I commenced to percuss the humerus, carefully supporting the ulna from refracture, and continued the operation every alternate day on twenty-five occasions with the effect of greatly enlarging the radius. The forearm became much stronger, with some amount of consolidation, but wanting in firmness; and, as the patient had excellent use of the arm, I did not advise further interference. The failure of complete consolidation in this case arose from the fractured ends having become absorbed, and consequently wanting in length to come in absolute contact.

The cases in which I have practised the foregoing method, though not numerous, afford a fair test of its value, more especially the first case, in the treatment of which I did not anticipate any success. During my professional career (in my own practice) I have frequently had delayed union, but only one case of non-union, that being the case *last* reported.

As to the cause of non-union, I am by no means satisfied that it arises from the want of skilful treatment or insufficient immobility, as I have frequently noticed cases that were thoroughly fixed and carefully watched delayed in union; while in another case, though receiving less attention possibly, a rapid union took place.

I have tried various methods; manipulation and friction to the ends of the fractured bones, and passing a tenotome between the ends of the bones but both of these methods are very unsatisfactory. I have also tried the method with bone, metal pegs, and screws, first having sawn the ends of the bones, using the screws or pegs with the intention of assisting immobility, and I am satisfied that the limb does better without the pegs than with them, having on frequent occasions operated without these accessories.

I have also operated on three occasions by a subcutaneous method, using an instrument which was chisel-shaped at the point, and file-cut on the rest of the blade, thus combining a knife and a rasp. This answered very well.

On one occasion also I operated upon a humerus where the bones were an inch short of being in apposition, and the limb had the appearance of an hour-glass contraction at the part where the bone was deficient. I operated upon this case by (what a ship-carpenter would call) "scarfing" the ends of the bones, with the result of a very excellent recovery.

Such a case as the latter would *not* be amenable to the treatment by percussion, but I am confident that the method of treatment by percussion will be found most useful for all ordinary cases of non-union, where the bones are in apposition and no interval of deficiency or any tissue exists between their ends.

Messrs Khroné and Seseman, of London, supply a very neatly-constructed copper-headed mallet which I designed for these operations.—*Manchester Med. and Surg. Reports*, 1876, p. 27.

38.—NEW OPERATION FOR UNUNITED FRACTURES.

By Dr. MATTHEW HILL, Bootle, Liverpool.

[The operation described in this paper might be described as "A new Modification of Dieffenbach's Operation." The latter consists in opening up the seat of fracture and driving ivory pegs into the ends of the bony fragments with the intention of exciting inflammatory action and consequent throwing out of reparative material.]

In many instances, especially in the femur and humerus, it is considered "advisable not to employ the seton, but to pass on at once to Dieffenbach's plan," as being attended with less danger. Such a case is the one I have now to relate.

Mr. W., æt. 35, a gentleman of untainted constitution and active habits, had his thigh broken by a railway collision at Wigan on the 25th of December, 1874. He sustained a few other superficial injuries, and was exposed for some hours in the frost and snow. The fracture, which occurred at the junction of the middle with the lower third of the femur, was "simple," and oblique in direction. It was carefully attended by an experienced surgeon at Wigan, who put him up on the long splint, afterwards on a MacIntyre, and finally in plaster-of-Paris bandage. At first all seemed to progress favourably, but at the end of seven or eight weeks he found the fracture still ununited, and requested a consultation upon the patient's case. As I had attended this gentleman before, I was summoned to Wigan, and after carefully examining all the details we made out that Mr. W.'s ununited fracture was mainly to be attributed to a hacking throat-cough (caused by an elongated uvula), which constantly shook his frame and jerked the fragments, more especially the upper. At the same time his bodily condition was soft and flabby from long confinement to bed, so much so that we considered it unadvisable to subject him to any operative procedure just then beyond snipping the elongated uvula and painting the throat with perchloride of iron, which very much checked the cough. As for the limb, a large piece of Cocking's poro-plastic felt was moulded into an admirable splint, quite enveloping the thigh, and when tightly strapped enabling him to get about on crutches, and even to bear a little pressure on the foot. After a couple of weeks' hobbling about in this way his health much improved, and he gained sufficient confidence and strength to come down to Liverpool and Bootle, just ten weeks after the accident. The case being now turned over to my care, I had him etherised, and proceeded to break up the false union by forcibly twisting and grating of the fractured surfaces against one another. After some weeks I found the result to be simply *nil*.

A consultation was now held with Mr. Reginald Harrison and Mr. Puzey, the latter of whom attended on behalf of the Railway Company, and we agreed to perform subcutaneous scarification of the ends of the bone, and to bend the knee forcibly, as it was getting very stiff; indeed, then and afterwards, we found reason to believe that the lower fragment was probably split longitudinally down to or near the articulation, but of this we could not be certain. The limb was then fixed in an improved apparatus of the MacIntyre kind, and left at rest. Again there was no local action set up except some pains in the knee-joint, and at the end of a further five weeks from the time of scarifying the pieces were as obstinately loose as ever. Finding now that something further must be done, and that the

most likely would be Dieffenbach's operation, I thought a good deal as to how it might be best performed so as to leave the fracture still in its "simple" state, and finally came to the conclusion that the pegging might be done subcutaneously, thus rendering this severe operation of the third class into the milder one of the first class. The tools required are an Archimedian drill-stock, a steel drill four or five inches long, and a few ivory stilettes of the same length and diameter (or slightly tapering) as the drill. The drill and stilettes are similarly graduated in half inches, and the ivories are, moreover, grooved like a director in order to slide along the drill. The *modus operandi* consists in entering the drill through a puncture made by a tenotome down to the bone; the depth of the soft parts is now read off by means of the graduations; then, if it be desired to bore into the bone to the depth of an inch, the drilling is proceeded with until the steel has penetrated an inch further than the original reading. The ivory stilette is now filed *half way through* an inch from the point, and after being soaked in carbolic oil is guided by its groove down alongside the drill to the brink of the perforation in the bone, from out of which the steel is next lifted, the ivory slipped into its place, hammered, and by a smart lateral movement broken off at the filed notch. The operation is completed by withdrawing the remainder of the stilette and sealing the puncture with a bit of lint and plaster. By carefully following the foregoing details it will be found that an inch peg is accurately placed in an inch hole, consequently there is no portion of it projecting into the flesh, and of this we may be certain by seeing before it is broken off that the reading on the ivory at the surface of the skin tallies with the previous reading on the drill, both being graduated alike. A further object is secured by the peg being grooved; a channel is thereby provided for the escape of fluids in the event of osteo-myelitis being set up, thereby avoiding the danger and suffering caused by the damming up in the bone of inflammatory fluids, as would necessarily be the case were the peg solid. If the fracture be oblique or the position of the fragments suitable the drilling may be pushed on into the other portion of the bone, after which a longer peg of ivory can be inserted to hold both pieces together; this, although difficult, is by no means impossible, and would require some slight modifications in the apparatus. The foregoing plan of the operation was submitted to, and kindly approved by, the gentlemen before mentioned; indeed, to Mr. Harrison I am indebted for much aid and valuable suggestions in maturing the details, and we tried it together on the dead subject before venturing it upon the living. All being in readiness, the operation was performed, in the manner just described, on the 21st of

last May, when we successfully inserted one peg in the upper and two in the lower fragment. One of the latter pegs was notched to break off one inch from the point as the others had been, but owing to a flaw in the ivory as well as being very deep and tightly gripped by the muscles it broke off about an inch and a half long instead, thus leaving about half an inch projecting above the bone. In spite of this the three punctures healed in twenty-four hours without the development of any pus or constitutional excitement then or afterwards. Thus far we demonstrated the safety and practicability of performing Dieffenbach's operation by the subcutaneous method, and I wish I could add, at least for the patient's sake, that our operation had achieved its intention of exciting local action in the bones, but after giving him plenty of time and rest the fracture is now as loose as ever. The pegs are not felt; in fact, at no time did they give rise to so much local action as we could have desired, and I cannot but think that in this case the patient's osseous system must be unusually tolerant of injury. The operation has failed in this case, the only one in which it has yet been tried, and is so far a discouragement to any further trial of it, for after all "nothing succeeds like success;" but it should be remembered that in this case Dieffenbach's own plan would have failed, and that in many after everything has been tried, amputation has been found necessary.

One point worthy of notice is that in case of the pegs' exciting suppurative inflammation and the formation of abscess requiring the laying-open of the fracture the patient is in no worse position as to the wound than the original operation requires; but it must be confessed that in these days of anti-septic surgery the exposure of bones and even of joints is not the formidable thing of the past. Respecting the apparent disappearance or absorption of the pegs in the foregoing case—for no trace of even the broken one which projected into the soft parts appears to be left—it is interesting to recall Mr. Savory's experiments on the absorption of dead bone ("Med.-Chir. Trans.," vol. xlvii., p. 103). From these it would appear that one essential towards absorption is firm contact, as in a peg tightly driven into a hole, one which is loosely placed not being affected. To sum up, the operation is, as it always has been, scientific from a physiological point of view; the new modification of it must be allowed to be practical from a mechanical as well as a physiological point of view; its utility requires to be further tested, and I trust will be notwithstanding the cloud of failure which overshadows its first trial. It may be expected to succeed in any case in which Dieffenbach's method promises success, and likewise to fail where that operation has hitherto failed.—*Liverpool and Manchester Medical and Surgical Reports*, 1876, p. 64.

39.—SHORTENING AND DEFORMITY OF THE FEMUR,
CONSEQUENT UPON FRACTURE, SUCCESSFULLY TREATED
BY RE-FRACTURE.—DR. CRUISE'S SPLINT.

By P. J. HAYES, Esq., Surgeon to the Mater Misericordiæ
Hospital, Dublin.

Cases demanding re-fracture of recently united bones are not of frequent occurrence, hence the following report may prove of sufficient interest to merit notice.

J. M., aged twenty-seven, on the 23rd of August, 1874, received a fracture of the right femur by falling from a car. The bone was broken in the upper part of the middle third, and though the fracture was simple, considerable swelling of the thigh took place before the services of a surgeon could be obtained. Then for some days it was found impossible to apply a splint or reduce deformity. Six days after the accident a Liston's thigh splint was employed, but it was necessary to use, in addition, a short anterior splint, in order to overcome the forward tendency of the upper fragment. The patient could not well bear the pressure of an efficient perineal band, and the gentlemen who had charge of the case determined to remove the splints, and replace them by a gypsum bandage. The splints were taken off on the 1st of October, but as there was some delay in obtaining suitable gypsum the patient was left with sandbags at either side of the limb for twenty-four hours. During the night he managed to twist the thigh, and on the following morning his medical attendant was disappointed to find considerable bowing outwards at the seat of fracture. Chloroform was administered, and the limb brought into as good a position as was possible; then the long splint was re-applied, but as a perineal band could not be borne, extension was kept up by means of a weight and pulley. This treatment was maintained during three weeks, and then the gypsum bandage was employed, and kept upon the limb for nearly three weeks longer. On the 11th November, whilst walking with crutches, the patient again received a hurt of the injured limb from a heavy fall, which was caused by one of the crutches becoming fixed between the bars of a cellar grating, and the surgeon who had attended him previously advised that he should be sent to the Mater Misericordiæ Hospital, where he was admitted on the 23rd November, under the care of Mr. Tyrrell, who found considerable bowing of the thigh in a direction forwards and outwards, as well as shortening of the limb by about two and a half inches.

Mr. Tyrrell determined to break through the callus, and use strong traction, but unfortunately, owing to indisposition, he was forced to relinquish for a time hospital attendance, and

having described the case to me, he requested that I should undertake the necessary treatment. On the 2nd of December the patient was brought fully under the influence of ether, and placed upon a mattress on the floor of the operation theatre. Dr. Cruise's apparatus for making extension in cases of dislocation was then applied to the limb, exactly as though the patient was suffering from a dislocation of the femur upon the dorsum of the ilium. the traction force was worked up to 125 lbs. and, with my knee against the seat of fracture, I was able to bring all my strength and weight to bear upon the convexity of the bowed thigh. After some minutes I found indications of yielding in the mass of callus, and a few more efforts enabled me to break through it completely. The fragments were disengaged, and considerable extension effected by increasing the traction to 132 lbs.

The patient was then removed to his bed, and sandbags placed at either side of the thigh. I thought it well to allow an elapse of twenty-four hours before using any splint, or subjecting the patient to additional disturbance. The man was in good spirits on the ensuing morning; he had slept well, and was not in pain; therefore I proceeded to apply a splint which Dr. Cruise kindly lent me, and which proved of great value in effecting and maintaining powerful and necessary extension during three and a half weeks after the operation.

It may be as well to describe this splint before proceeding with particulars of the case. The splint consists of a long wooden portion, to extend from the axilla down beyond the foot; projecting from the inner surface of this splint, close to the lower end, is a thick stout block, perforated from above downwards, and giving passage to a screw twelve inches long, the thread of which fits a spiral groove in the aperture of the block; attached to the upper extremity of the long screw is a cross piece, with projecting hooks, constructed to catch the loop of a Pancoast's stirrup; fixed near to the upper end of the long splint is an iron bar, doubly bent at right angles, so as, when applied, to ascend on one side of the patient's thorax or abdomen, cross in front, and descend on the other side; each end of this bar has in it an opening, through which the posterior part of a perineal band can be passed, the other part being tied to the middle portion of the bar which crosses in front of the patient's body. The splint was used in the following manner:—The patient's leg, from the knee down, was shaved and well washed with vinegar and water, then six strips of soap plaster were caused to adhere one over another; thus united they formed an adhesive strap, about two inches wide, and sufficiently long to extend from the inner side of the knee down the leg, form a good loop below the heel and arch of the

foot, and then reach up the leg to the outer side of the knee. Some cotton wadding having been applied over and behind the malleoli, the strap was secured by the exposed surface of soap plaster to the sides of the leg, and a piece of wood three inches and a half long was placed transversely in the loop below the foot; strips of soap plaster were crossed round the leg, so as to afford additional security against the risk of the stirrup slipping from its position under the strain of traction.

Two perineal bands, right and left, were prepared with the view of relaxing one and tightening the other whenever the skin might be likely to suffer from prolonged pressure. The splint was padded, then placed along the outside of the fractured limb, connected below to the stirrup, and above with the perineal band on the right side. Steady working of the screw effected force for extension and counter-extension, and the amount of force employed was regulated by the feelings of the patient, who, being a very sensible, intelligent man, did all in his power to second efforts for his improvement.

The progress of the case was very satisfactory, owing to the care which my resident pupils, Messrs. Flanagan and Hartigan, gave to the working of the apparatus. Three and a half weeks after re-fracture the limb was put up in a gypsum bandage, and the patient allowed to go about on crutches. Three weeks later the gypsum was removed, and the patient left hospital on the 24th of January in the present year, with a straight limb, shorter than its fellow by only three-quarters of an inch.

An interesting case of re-fracture of the femur five months after union was published by Mr. Butcher in the number of this Journal for November, 1874. In that instance very considerable force was required to break the callus. In the London Medical Record for March 31st, 1875, Mr. Bellamy refers to a valuable paper by Professor Nussbaum, of Munich, in which the subject of re-fracture is dealt with, and especial mention made of the treatment to be adopted when the definitive callus has become harder and stronger than normal bone. Thus Langenbeck makes a small wound in the soft parts, then bores a hole through the callus at an angle, and with a fine key-hole saw cuts from this hole right and left until only thin bridges of surface bone remain undivided. After a time healing of the wound in the soft parts takes place, and then moderate force effects simple fracture of the bridges of bone.

Szymanowsky cuts through the soft parts, and saws a wedge-shaped piece out of the callus, three parts of the thickness of which he removes; then, after healing of the soft parts, he easily breaks through the remaining portion of callus.

Nussbaum's proceeding is to chisel through about three-quarters of the thickness of the bone, allow the superficial

wound to heal, and afterwards to fracture the undivided portion of bone. He avoids sawdust and *débris*, which might retard healing, by using a fine sharp cabinet-maker's chisel, instead of either saw or drill.

In most cases, however, the surgeon will find he has not only to break through the osseous connexion, but also to overcome the resistance and contraction of fibrous tissue developed around the seat of fracture. In the case I have recorded such tissue was abundant, and, owing to its tough and unyielding nature, I found it impossible to restore normal length of the limb. I received the following communication from Dr. Cruise

since the above report was written. The accompanying woodcut will render distinct portions of my description:—

“I send you a woodcut of the splint for fractured femur which I devised some years ago.

“Its advantage amounts to this—that by means of the iron arch A the surgeon has the opportunity of making counter-extension from either side of the perineum at will, and under these circumstances it is impossible, provided ordinary care is taken, for the perineum to become tender, or unable to bear the pressure of counter-extension.

“Your recent case of re-fracture of a deformed femur exhibited the necessity of forcible extension, and the case for which I devised this modification of Desault's long splint proves the same fact. My patient was a man named John Reilly, aged forty, who was brought to the Mater Misericordiæ Hospital June 25th, 1863, with an oblique fracture of the right femur, and also a fracture of the corresponding humerus. The latter was easily managed, but the fracture of the femur offered great difficulties. It was remarkably oblique, and the patient being a powerful muscular man, shortening took place, and increased, despite my best efforts, because whenever I made extension sufficient to



overcome the deformity, the perineum became tender, and after a day or so the strap became unendurable. In this difficulty I thought of the iron arch A, had it made by a blacksmith in my neighbourhood, and I attached it, as the woodcut shows, to the long splint by means of four strong screws.

“From the time I used it I experienced no further trouble. Whenever one side of the perineum became tender I relaxed the strap there, and tightened up that on the opposite side. In this way counter-extension caused no annoyance, the femur united without any appreciable deformity, and the man left the hospital perfectly well on the 29th of September, 1863. .

“Of the superiority of this method I think there is no need to speak.

“The idea of making counter-extension from either side of the perineum at will is not original with me. Several American surgeons have done so, and notably the brothers Burge, of Brooklyn, and Lente, of Cold Springs, New York. For Lente’s apparatus I refer you to Hamilton on ‘Fractures and Dislocations,’ page 423.

“The iron arch I have suggested has, nevertheless, the advantages of great simplicity and cheapness, may be made in an hour for any long splint, and makes the counter-extension less oblique and, therefore, less annoying than any apparatus I know of.”—*Dublin Journal of Medical Sciences*, Oct. 1875, p. 289.

40.—CASES OF EXCISION OF THE KNEE-JOINT.

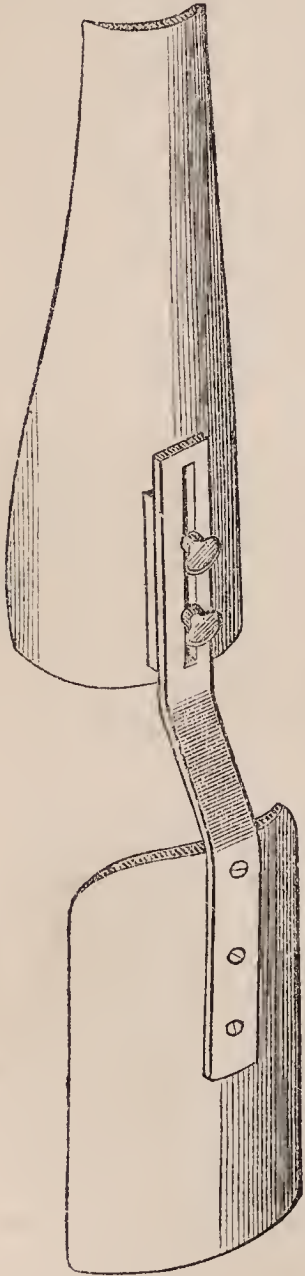
By P. J. HAYES, Esq., Surgeon to the Mater Misericordiæ Hospital, Dublin.

The great act of surgical conservatism, excision of the knee-joint, is no longer looked upon by those who really know much about it as an operation of doubtful value or extreme risk, but it certainly is a measure to be adopted only after grave consideration and due estimation of the circumstances belonging to each case which may seem to demand a proceeding so serious. In the Irish Hospital Gazette, January 1st, 1874, I published some conclusions arrived at from my previous experience of excision cases. I now report particulars of two successful excisions, as they serve to illustrate points which ought to weigh in favour of excision, instead of amputation, when it comes to be a question which operation is to be selected, or advised by the surgeon.

Case 1.—C. H., a girl, aged eighteen years, gave the following history:—She enjoyed good health until about eight years ago, when she was thrown from an outside car, and fell upon the left knee. The pain at the time of the injury was so severe

as to cause fainting and sickness of stomach. Two or three days after the accident she was able to go about, though the joint remained swollen and rather painful, but at the end of a month she became unable to use the limb, in consequence of extreme tenderness, increased swelling, and weakness of the knee. A doctor was then consulted, who said there had been dislocation of the joint; he extended the leg, and advised perfect rest. About a week afterwards she gave the knee a twist, or jerk, but did not mind it. The doctor, however, appeared to think the "dislocation" had recurred; he, however, used external application of iodine, and enjoined complete rest. After a time the patient could walk, but she was lame, and felt a constant dull pain in the knee. This was rendered worse by exercise. The pain became very severe some two years since, but was relieved for a while by blistering, &c. Soon, however, painful startings set in, accompanied by hectic fever, and in July, 1874, she came to Dublin, and was admitted to the Mater Misericordiæ Hospital, under the care of Dr. Cruise. In spite of the most judicious treatment little change for the better occurred, and as she had a sharp attack of erysipelas, consequent upon the use of counter-irritation, she was advised to return to the country, in order that her general health might undergo improvement. She was again admitted to hospital at the close of September, and as other treatment had not been productive of benefit, on the 22nd of October Dr. Cruise applied the actual cautery freely to the skin at either side of, as also above and below, the patella. This seemed to produce good effect for a time, but in December the pain returned with renewed violence, the nocturnal startings became more frequent and distressing, and urgent hectic symptoms appeared; therefore, in consultation, it was considered advisable to recommend excision of the affected joint, and my colleague complimented me by kindly placing in my hands the further conduct of the case. The circumference of the left knee now exceeded that of the right by more than an inch and a half; the swelling was almost uniform, and gave the joint an oval appearance; the the patella was movable; pressure applied over the tuberosities of the tibia, or condyles of the femur, caused considerable pain, and sleep could only be produced by hypodermic injections of morphia. On the 24th of January, in the present year, I excised the affected joint, ether was the anæsthetic used, and hemorrhage prevented by Esmarch's method. After the operation the raw surface was well sponged with a solution of chloride of zinc (gr. 20 ad. ℥i.), and the wound closed, save at either angle or corner, by sutures of carbolised catgut. The limb was then put up in the following manner:—Two flannel bandages, one over the other, were evenly applied from the

toes up to the level of the anterior tibial tuberosity; the thigh was bandaged also with flannel from the groin down to within about two inches of the lower end of the femur. A splint, specially designed by myself, consisting of two concave pieces of wood—one to receive and fit the posterior surface of the leg, from the upper part of the calf to within two and a half inches of the ankle; and the second, to receive the posterior surface of nearly the lower two-thirds of the thigh—the two portions



of wood being connected behind the popliteal region by a strong flat piece of steel, with an oblique step in it, so as to keep the leg portion of the splint one inch in advance of the thick portion. Thumb-screws and an oblong slot in this steel strap enable the surgeon to increase or diminish the space between the wooden portions of the splint, according to requirements. The leg and thigh pieces having been suitably padded, the limb was laid upon them, and the interval between them regulated to about three inches. A small splint of poro-plastic substance was next padded, and laid over the anterior surface of the thigh, and then a gypsum bandage carried from the toes to the upper edge of the leg bandage (flannel), brought behind the steel band and popliteal space, and carried on round the thigh to the highest level of the splint. This was repeated until a strong casing of gypsum rendered the position of the leg and thigh perfectly secure. Cotton wadding, saturated with melted paraffin, was inserted into the space between the skin of the popliteal region and the steel portion of the splint; also every chink in the gypsum bandage near the wound was painted over with paraffin, whilst the skin in the neighbourhood of the wound remained quite uncovered, and free for the application of dressings, &c. A piece of oiled lint was laid lightly across the wound, and the

patient removed to bed. The soft tissues united by adhesion, not a drop of pus ever formed, and on the 14th of February the patient was able to be dressed and carried from her bed to a sofa which was on one of the corridors. By March 18th the patient was able to walk with crutches, as the splint was re-

moved, and replaced by a simple gypsum bandage. There was excellent union. Within the next fortnight she could walk without any assistance from crutch or stick, and left the hospital on the 1st of May with a perfectly straight strong limb, an inch and three-quarters shorter than its fellow.

As regards pulse and temperature, the highest reading of each was on the third day after operation, when the former was 120 in the minute, and the latter reached 101° F. On the tenth day both were down to the normal standard.

The total amount of bone removed was an inch and a half, measured from above downwards. The disease had completely destroyed the synovial membrane, and there was ulceration of the cartilage, both upon the outer condyle of the femur and corresponding facet upon the patella. The cartilage for a considerable area around the ulcerated points was of a rose-red hue, owing to sub-cartilaginous osteitis.

Case 2.—M. C., a girl between seventeen and eighteen years of age, was admitted to the Mater Misericordiæ Hospital, under the care of Mr. Tyrrell, on the 2nd November, 1874.

History.—Nearly five years back, when at school, she fell from a high table, and her left knee struck upon a hot-water pipe. There was no wound, but she fainted from excessive pain. After the accident she remained in bed for some days, as the joint became very painful and swollen, but at the end of a week she could walk a little, and seven weeks later she returned to the day-school. At this time when the knee was at rest the girl was free from pain. It remained swollen, however, and after much walking the pain returned, so almost every night she felt it stiff and tender. Matters gradually changed for the worse, and after the lapse of two years the patient could not walk, as the least motion produced severe articular pain, and at the same time most distressing nocturnal startings of the limb set in. A medical man was at length consulted. He strapped the affected joint, and applied a suitable splint; of course enjoining strict repose. Under this treatment great relief from pain was experienced, but in other respects the condition of the joint seemed not to undergo any change. After a time the pain and startings returned, consequently the girl was brought to Dublin.

It is unnecessary to state the constitutional and local treatment employed by Mr. Tyrrell. The knee was in such a state that all his efforts were directed to allay existing inflammation, and keep the limb in proper position until ankylosis should take place. By the end of December the girl's sufferings were severe indeed. Splints and all extending media had to be removed, as she could only tolerate the semi-flexed position for the knee. There was now a tendency towards backward dislocation of

the tibia, and it became manifest that excision of the knee was the proceeding most likely to save the patient's health and limb. Mr. Tyrrell being unable to attend at the time, I undertook charge of the case, and on the 27th of January I performed the operation, removing in all one inch and five lines of bone (measured from above downwards). No hemorrhage occurred, owing to the employment of Esmarch's bandage, after the removal of which three small vessels required to be twisted. The wound was sponged out with chloride of zinc solution, and then the edges brought together by means of catgut sutures. The limb was put up in the manner already described (see preceding case), and nothing could be more satisfactory than the position and security of the bones. Five days after the operation erysipelas attacked the skin about the wound, and necessitated division of the sutures; but, save that all chance of union by adhesion was lost, the effect was not unfortunate, for the inflammation rapidly subsided, and soon granulations filled up the interval between the flaps, suppuration being extremely slight and of very short duration. The temperature reached 102° F. at the onset of the erysipelas, and during the first eight days the pulse ranged from 100 to 130 in the minute. After that temperature and pulse fell to the normal condition. The girl was up a month after the operation, and as the bandage did not now tightly embrace leg and thigh, she could rotate the entire limb within its casing. On the 18th March the splint was removed, and a strong carefully applied gypsum bandage substituted for it, the patient being allowed to go about on crutches. Union soon became very strong, all swelling subsided, and the shortening was only an inch and one-third. The result is quite as perfect as in the case of C. H. Permission for her discharge from hospital was given May 1st, but she was kept on longer, in order that photographs of the limb might be taken. The joint, when examined, showed complete destruction of encrusting cartilage, erosion of part of the head of the tibia, and uniform pulpy degeneration of the synovial membranes.

The points to which I would call attention in the foregoing cases, as indicating the propriety of excision, are—1st. The occurrence of progressive articular disease after injury, and not because of constitutional fault; and 2nd. The comparatively slow advance of the disease, and total absence of suppuration.

As regards the operation, my usual practice is to moderately flex the leg upon the thigh, and carry an incision across the ligamentum patellæ, from the posterior edge of one femoral condyle to a corresponding point on the other (of course, when the limb is extended this incision will form a curve convex downwards). I remove the patella, and saw off no more of

femur or tibia than is absolutely necessary, but I pare away every trace of articular cartilage, and remove as completely as possible diseased synovial membrane and crude formations. It is confessedly a difficult matter to overcome the tendency which the lower end of the femur has to project on a plane in front of the greater part of the upper end of the tibia; an outward bowing of the limb also requires to be guarded against. By means of the splint I have described it is alike easy to arrange and retain the bones in perfect position, whilst in other respects all the conditions favourable to early union of the tissues are secured, with comfort to the patient, and with the least possible amount of trouble to the surgeon.—*Dublin Journal of Med. Science*, Oct. 1875, p. 294.

41.—THE CARBOLISED CATGUT LIGATURE.

By the EDITOR of the MEDICAL TIMES AND GAZETTE.

In considering the use of the catgut ligature for tying arteries in their continuity, the question presents itself—Is carbolic acid a necessary agent in the preparation of the catgut? In order to answer this question, we may briefly consider the mode of preparing this so-called *catgut*. The material is really a part of the peritoneum of the sheep, with some fibres of unstriped muscle. This, having been properly cut into lengths and sizes for ligatures, might be simply dried and used, or used fresh, but in either of these conditions it is slippery, hard to tie, and when tied apt to stretch, and the knot to slip. Mr. Lister anxious to procure a ligature which should melt away and be absorbed without acting as a foreign body in the wound, and looking to the somewhat unsatisfactory experiences of Astley Cooper and others, found that by a special preparation this catgut was so altered as to become a firm and useful ligature. If it is suspended in an emulsion of oil and water, during the first few days it becomes dull and opaque, but then a remarkable change occurs: it becomes clear, bright, and hard, and capable of being tied without stretching and slipping. In order to attain this changed condition it is necessary to keep it suspended in the emulsion for about two months, the bottom of the vessel being so arranged that the water, as it separates from the oil, falls down clear of the suspended catgut. It will be ready in two months, but it goes on improving if kept in the emulsion for a much longer period. In order to make a very fine emulsion, the water was mixed with something which the oil would take from it—say spirits of wine—and the water was thus left suspended amongst the oil in very minute drops. Mr Lister wished to have an antiseptic ligature, and he found that carbolic acid had the requisite properties for forming

along with water and oil the required fine emulsion. Hence the *carbolic* catgut ligature.

This answers our question; but it seems probable that if nothing to prevent decomposition was mixed with the emulsion, the animal tissue kept in it for a considerable length of time would be apt to become putrid, and if it did not at the same time lose its special properties, still it would, from its putridity, be very unfit for surgical purposes.

The next question—Is the catgut, carbolic or not, a safe ligature for application to arteries in their continuity?—divides itself into several heads, which we may endeavour to answer from the experiences related during the discussion at the Clinical Society.

If properly prepared, the general experience seems to point to the fact that it is safe, provided the artery be in a fit state for any surgical interference. But an element of uncertainty arises, as some surgeons in tying it divide the internal coat of the artery, while the majority find that they do not. We believe that if sufficient force be used to divide the internal coat, with the additional bruising to the outer coats that this implies, the catgut will be found inferior to silk, because from its liability to softening and absorption it does not maintain a firm hold on the artery nearly so long as silk, and consequently does not give Nature so long a time to complete her safeguards against secondary hemorrhage.

On the other hand, it seems that the ligature may be so used as to cause ulceration externally, and yet relax so soon as to leave the artery pervious—a very serious state of things.

The remedy to such misfortunes appears to us, however, to lie in a little more experience as to the proper amount of force to employ in tying it, so as effectually to occlude the artery and allow the formation of clot without dividing the internal coat on the one hand, or causing ulceration or sloughing of the external coat on the other.

So far we have kept altogether out of sight the question of its use in aseptic surgery, for which it was introduced in its present form by Mr. Lister.

In his hands, and in those of other surgeons who thoroughly carry out his method, it has been found most trustworthy both for ligature of arteries in their continuity, and also for the ordinary tying of divided arteries; the number of cases in which secondary hemorrhage has occurred being so very small, that, looking to the varying nature of the cases, it may fairly be pronounced perfectly successful. Nor must we forget that Mr. Lister only advocates its use in aseptic cases, and that some, at any rate, of the cases we have just alluded to, in which secondary hemorrhage has occurred, have been septic ones from some failure in the details of his method.

In speaking of the preparation of the catgut, we have alluded to the probability that it would become putrid if no antiseptic agent were used; and now we come to the question—Do fluids in a putrid state act in a different manner on the prepared gut to the natural secretions of a wound in an aseptic state? We are inclined to think that if pieces of the same catgut were placed in the secretions of a wound which was distinctly septic, and in those of a wound which was as distinctly aseptic, a marked difference would be found in the state of the material when withdrawn after a few days.

Evidence on this point would not be difficult to obtain, and would have a most important influence on the settlement of this question. Certainly clinical experience points to some such theory to account for the uncertainty in the results obtained by those who merely use the ligature, as compared with the certainty of results obtained by those who use it along with Lister's method.—*Med. Times and Gazette*, Nov. 27, 1875, p. 600.

42.—ON THE EMPLOYMENT OF CARBOLISED CATGUT AS A LIGATURE IN AMPUTATIONS AND OTHER MAJOR AMPUTATIONS.

By Dr. A. W. NANKIVELL, Resident Surgeon to St. Bartholomew's Hospital, Chatham.

[Mr. Nankivell has extensively tried carbolised catgut as a ligature in all kinds of operations. Its use was suggested to him by the occurrence of a fatal case of secondary hemorrhage from accidental traction on a hempen ligature.]

The carbolised gut that we have been in the habit of using in this hospital is to be obtained of Messrs. Macfarlan and Co., Barge-yard, Bucklersbury. There are five sizes, from No. 0, which is the thinnest, to No. 4, the thickest. I have employed only two sizes—viz., Nos. 3 and 4, the latter number being quite strong enough for the deligation of any large artery, such as the femoral or brachial, in continuity or on the surface of a stump. No. 3 is of the proper dimensions for the radial, ulnar, and similar vessels. The thinner gut may be used for the arteries in amputations of the fingers and toes, but too many sizes lead to confusion, and I have therefore been content to employ Nos. 3 and 4.

The prepared catgut is sold in hanks steeped in the carbolised oil. It should always be kept in the liquid in stoppered bottles, and only taken out when required, for if this be not done, it is apt to become rough and brittle. When about to be used—as, for instance, in an amputation of the thigh—an assistant should cut a piece of about four inches in length for the ligature of

the main vessel from No. 4. The remaining arteries should be tied with No. 3. I find that about eight inches of the gut is enough to secure four vessels. The catgut should not be too old, for I have observed that some that I kept in stock for about two years became far more brittle than when more recently prepared. In tying vessels in an open wound I am very particular in separating the artery from its sheath and accompanying veins before proceeding to apply the ligature, and care should be taken to avoid all jerking in tightening the catgut. When the material is sound, it will bear steady pressure without breaking. It is impossible to give any directions as to the amount of force to be used in tying an artery; each surgeon must judge for himself, and acquire it by habit. I rarely now, if ever, break a sound piece of catgut when applying it to a vessel. If a small artery cannot be separated from the surrounding tissues, it is sometimes necessary to use a piece of No. 4 to tie the vessel and neighbouring structures with sufficient force. The ligatures are to have both ends cut off quite close to the artery. I may, perhaps, mention that prepared catgut is not suitable, in my experience, for sutures in major operations, especially if there be much tension on the parts; it melts away before firm union of the skin has taken place. I am informed, however, that it is useful as a suture in wounds and operations about the eyelids.

The following is a list of the operations in which the vessels have been secured with the carbolised catgut:—

Amputation of thigh	22
„ „ leg	10
„ „ foot	6
„ „ arm	5
„ „ hand	2
„ „ breast	10
Excision of hip	2
„ elbow	2
Castration	3
Ligature of radial artery	1
„ ulnar	1

In the above table I have excluded lithotomy, herniotomy, and the five instances of ligature of the femoral artery which have already appeared in the *Lancet*, as also all minor surgery. These operations have been performed on both sexes,—on children, adults, and old people; the oldest patient being seventy-six years of age, with calcareous degeneration of the arteries and sloughing of the flaps.

Some of the cases did remarkably well, but we have had our share of erysipelas, septicæmia, and sloughing in amputations, yet notwithstanding the unfavourable state of some of the

operations the catgut held firm in all cases save one, and we have never had but one instance of secondary hemorrhage in this hospital since we employed this ligature. It is with especial reference to this point that I have written this paper, as I wish to show what our experience of the use of the carbolised catgut has been in preventing the occurrence of secondary hemorrhage. The only case in which it did occur was in an amputation of the foot for incurable ulceration, the result of frost-bite. Hemorrhage took place from a portion of the diseased tissue, which of necessity was left behind after the operation. It was speedily arrested by the application of a compress, and the man made a good recovery. It has appeared to me that in intractable ulceration resulting from burns and frost-bite there is a tendency to secondary hemorrhage, whatever ligature be employed. My confidence in the catgut is at present unshaken, and I hope that other surgeons will give it a full trial in open wounds and arteries in their continuity and publish the results of their experience.

In conclusion, I will venture to claim the following advantages for the catgut—(1) immediate closure of the deeper parts of a wound; (2) absence of sinuous tracts; (3) diminution of subsequent bleeding, or rather its entire absence in consequence of the free use of ligatures to bleeding points during the operation; (4) absence of danger from accidental traction on a ligature; (5) above all, the almost entire avoidance of risk from secondary hemorrhage as illustrated by the results of the cases given above.—*Lancet*, Feb. 19, 1876, p. 278.

43.—ON AQUAPUNCTURE.

By R. CLEMENT LUCAS, Esq., Assistant-Surgeon to Guy's Hospital.

In a paper read before the Scientific Congress at Nantes, in August last, Dr. Léopold Lafitte drew attention to the value of the subcutaneous injection of pure water for the relief of pain. He states that he was induced to try this simple remedy from having observed, when in Paris in 1872, the good effects produced by it in a case of acute rheumatism under the care of M. Dieulafoy. In the opinion of the author of the paper it is an expedient of great value, and he mentions many cases in which he had adopted it with marked success; one especially, a case of acute lumbago, was immediately relieved by the hypodermic injection of two grammes of pure water. Dr. Lafitte refers to an article in the "*Nouveau Dictionnaire de Médecine et de Chirurgie Pratiques*," entitled *Douleur*, by M. Georges Dieulafoy, as containing the only written account of this mode of treatment. In this essay, published in 1869, Dr. Dieulafoy

concedes the credit of originating the treatment to M. Potain, and speaks enthusiastically of its efficacy. Indeed, after dismissing all other methods for the relief of pain in a few lines, he devotes a whole page to the discussion of its merits. "D'après les expériences très nombreuses, que nous avons recueillies," he says, "voici ce qu'on peut avancer. Quand un individu est atteint d'une douleur, quelque soit son siège et quelle soit sa nature, on peut sans inconvénient combattre la douleur par les injections d'eau, et dans un grand majorité des cas le résultat est immédiat."

Subsequent to the publication of Dr. Lafitte's paper, Dr. Lélut has given his experience of the same treatment in a letter to *L'Union Médicale* of October 5th, 1875. His short communication is rendered interesting by an account of the incident which led him unwittingly to employ so simple an expedient. His servant had by accident overturned a bottle containing morphia in solution, which had been left upon his desk, and, wishing to conceal her misfortune, refilled the bottle with water. The following day Dr. Lélut employed the liquid from the bottle for a subcutaneous injection in a patient suffering from sciatica, in whose case he had previously injected morphia. The result was that the patient was relieved, and delighted to find the pain removed without the nausea and sickness that had been excited on former occasions. Astonished at the different effect produced by this injection, Dr. Lélut was led to examine the contents of the bottle, and was surprised to find that it contained only pure water. He repeated the experiment upon other patients, and found that it invariably gave them relief without inducing the unpleasant nausea caused by morphia.

Dr. de Labordette and others have also recorded their experience of aquapuncture in the same journal. A notice of Dr. Lélut's communication appeared in the *British Medical Journal* of November 27th, 1875; and was followed in the succeeding number by letters from Drs. Burney Yeo and Griffith claiming priority in the hypodermic use of water for the relief of pain. Both these gentlemen mention cases in which they resorted to this expedient with success in 1868. It is probable that this simple experiment has occurred to the minds of not a few. I can, for my own part, testify to its having been a common practice among the dressers at Guy's Hospital in 1867, who, in cases of supposed malingering or imaginary pain, were in the habit of substituting water for morphia. The relief that not unfrequently followed this practice used to be regarded by them as proof of the imaginary nature of the complaint—a deduction which the systematic experiments of MM. Potain and Dieulafoy would seem to prove incorrect.

In the following case of sciatica the subcutaneous injection of water was employed at my suggestion, and seemed to bring about a very rapid cure. The case is reported by Mr. Grimwood, and was under the care of Dr. Pye-Smith, to whom I am indebted for permission to publish it.

W. M., aged fifty, a labourer, was admitted into Stephen ward, Guy's Hospital, on January 6th, 1876. He was born of a healthy family, and has generally enjoyed good health, but at the age of sixteen he suffered from an attack of rheumatic fever. His father died of dropsy. He is a married man, and has four sisters living and healthy.

About twelve months ago, when working at the butts at Woolwich Arsenal, he one day got wet through. On the following day he went to work as usual, and worked until dinner-time, but on attempting to rise after dinner he experienced a severe pain in the loins, which compelled him to go home to bed. He was attended by a medical man, and recovered after a time sufficiently to resume work of a lighter kind; but he did not lose entirely the pain in his left loin. He noticed that whenever he strained his left foot he was seized with cramp and suffered from a sensation as of pins and needles running up his leg. For about a month he has suffered from severe pain in his left hip, which is gnawing and continuous, but comes on sharper at some times than at others; it never entirely ceases. He also suffers at intervals from pain in the knee and on the dorsum of the left foot.

He is a tall, strongly-built man, with a ruddy countenance, but bears a pinched and anxious expression. He lies upon his back with his left leg drawn up and adducted, a position which gives him the greatest ease from pain. The percussion-note over the chest is good and the breath sounds are normal; the heart-sounds are indistinct but clear; liver and spleen normal; tongue clean; appetite good; bowels regular. His urine yields a slight deposit, but contains no albumen or sugar; sp. gr. 1030. Temperature normal. He was ordered a mixture containing magnesia and sulphate of magnesia, to be taken twice a day, and a Dover's pill every night.

Jan. 11th. The patient has not experienced any relief from the pain. A blister four inches square was applied over the sacro-iliac articulation, where the chief pain seems to be situated, and the following mixture was ordered to be taken three times a day:—Iodide of potassium, ten grains; bicarbonate of potash, ten grains; infusion of calumba, one ounce.

13th. Patient has less pain in the hip, but has suffered pain in the leg and ankle this morning.

16th. The pain in the leg and ankle is no better. A slight rash has come out upon his face, probably caused by the iodide.

of potassium. He feels well in health; temperature normal. Ordered a subcutaneous injection of a quarter of a grain of morphia every night.

17th. The patient is better. He says he has no pain in the hip or thigh, and has pain in the foot and ankle, which he can move about a little.

18th. A blister four inches in length by one in breadth was ordered to be applied along the course of the great sciatic nerve.

21st. Patient is better. The pain is confined to the foot and half-way up the leg; it affects the big toe and the two next. He has had no coldness of the skin since the use of morphia injections, and says he can move the leg more freely.

24th. The pain is less in the foot, and the patient can bend the toes, which he was unable to do before. He complains of pain of a darting character, which shoots from the heel into the the groin. He has a slight headache to-day.

25th. The pain in the hip and foot having returned, Dr. Pye-Smith desired that the patient should be examined by Mr. Lucas, who gave the following report: "The left leg is semi-flexed and adducted. The muscles of the left leg and thigh, and those of the gluteal region on that side, are considerably wasted. The left leg below the knee measured three-quarters of an inch less than the right on the same level. No pain is excited by percussing the heel or great trochanter. The movements of the hip-joint are quite free, pain only being caused by extreme flexion; and this is probably due to stretching of the great sciatic nerve, as he complains at the same time of a sensation of pins and needles down the back of his leg and foot. There is no thickening about the hip. There are some enlarged glands in the groin, produced, I have no doubt, by the irritation of the blisters. I believe the symptoms have their origin in the great sciatic nerve, and should recommend the subcutaneous injection of half a drachm of water three times a day."

The injections, which were of cold water and introduced into the arm, were given as follows: Jan. 25th, one at 9 p.m.; 26th, one at 4 p.m., and one at 9 p.m.; 27th, one at 10 a.m., a second at 4 p.m., and a third at 9 p.m.; 28th, one at 10 a.m.

He experienced relief from the first injections, and on the 28th was sitting up and quite free from pain. He says the injections first caused pain at the site of introduction, and were followed after a time by a warm perspiration and relief of the symptoms in the hip and leg. The injections were omitted on this day, and the following mixture was ordered to be taken three times a day: ten minims of dilute sulphuric acid, one drachm of compound tincture of cinchona, and an ounce of decoction of cinchona.

31st. The patient has had no return of the pain. The glands in the groin are still slightly swollen. He left the hospital to-day, feeling quite well.

Remarks.—There can be no doubt that this man's sufferings were perfectly genuine. There was wasting of the muscles of the gluteal region, as well as of the leg and thigh of the affected side, and the leg was semiflexed and adducted. Moreover, the percussion and manipulation, to which he was subjected, evoked from him none of the exaggerated complaints that would most certainly have emanated from a malingerer; and, further, he throughout expressed himself as being desirous of returning to his employment.

It will be seen from the report that he had been severely blistered, had taken iodide of potassium regularly, and received an injection of morphia every night for some time; and under this combined treatment he had improved. But a return or increase in the pain induced Dr. Pye-Smith to ask me to examine the patient for more substantial disease. The effect of the injection of water was, according to the patient, to cause severe pain at the seat of introduction, followed by perspiration and relief of pain in the affected limb. It may be, as Dr. Pye-Smith suggested, that the cure had already been wellnigh effected, and that the injections simply distracted the patient's attention from the part. I intended that the injections should be inserted over the seat of pain, but it is perhaps as well that the misunderstanding which led to the introduction of the injections into the arm should have taken place, although it renders more difficult any attempt to explain the manner in which relief was brought about. Dr. Lafitte, injecting always over the painful spot, accounts for the relief afforded by supposing that the fluid introduced compresses the peripheral extremities of the painful nerves, and so brings about their temporary paralysis. It is evident that no such explanation is applicable to the foregoing case, as the injections were introduced at a distance from the seat of pain. There remain but two other ways of explaining the effects produced: either the local irritation causes such central excitement as to dull the perception of the sensorium to lesser pains, or the effect is throughout a mental one, which may be compared to the relief sufferers from toothache so often obtain on approaching the house of a dentist. The explanation of the result, however, is of less consequence than the result itself, and may be allowed to wait until a certain consistency of effect has been established as occurring from the subcutaneous use of water. It is scarcely to be expected that all pains may be equally relieved by this one method, as Dr. Dieulafoy asserts. Numerous failures must surely occur; and, if I might forecast the verdict of the future, I would anti-

icipate that nerve-pains—neuralgias, sciatica, and the like—are more likely to be relieved by the remedy in question than the sufferings incident to chronic progressive diseases, such as cancer. As to the manner of injection and quantity of fluid to be introduced, Dr. Dieulafoy recommends that the seat of greatest pain should be searched for, and eight or ten drops of water be there injected. This, he says, may be repeated ten, twelve, or fifteen times at one sitting, if necessary. Dr. Lafitte, in a letter to *l'Union Médicale* of Oct. 5th, 1875, says that he injects two grammes and then waits a couple of minutes, and if the pain be not relieved he then injects two grammes more. Even five or six syringefuls may be sometimes injected. He thinks that less than two grammes or more than ten should never be injected. He further insists strongly on the necessity of injecting over the painful part: “Le seul point important à noter et à retenir c'est de faire l'injection *au point douloureux*. Tout le secret d'une bonne opération est là.” The operation itself is so simple and so entirely free from danger that positive proof or disproof of its efficacy in relieving pain ought soon to be accumulated.

In conclusion, if the foregoing case should lead to an impartial and systematic investigation of the value of aquapuncture, in England, the object of its publication will be accomplished.—*Lancet*, March 4, 1876, p. 344.

44.—ON THE ELECTROLYTIC DISPERSION OF TUMOURS.

By Dr. JULIUS ALTHAUS, Physician to the Hospital for Diseases of the Nervous System.

There are now so many and effective modes of treating the various forms of morbid growths which are met with in practice, that it would hardly be allowable to add another means to those already at the disposal of the surgeon, unless it could be shown that it possessed peculiar advantages of its own over other methods used for the destruction of these growths, or that it was applicable to such forms of tumours as are otherwise unmanageable or rebellious to treatment.

I will, therefore, only say that in electrolysis the constant voltaic current is conveyed to the tumours by means of gold needles, which act either in fixed rows, or singly in any direction that may be required by means of the *serres-fines* conductor; that either the influence of the negative or of the positive, or of both poles simultaneously, is utilised; and that the effects are not owing to the development of heat, and have consequently nothing to do with the galvanic cautery.

In cases where only the negative pole is used, as, for instance, in bronchocele, the operation is not painful if properly performed;

but, where both poles have to be inserted into the tissues, as in *nævus*, subaceous tumours, &c., a good deal of pain is generally experienced by the patient, and chloroform or some other anæsthetic may, therefore, be used under such circumstances if thought advisable.

1. *Nævus*.—One of the commonest tumours met with in practice is *nævus* of the face or scalp. Although this is not, as a rule, dangerous to life, it is nevertheless desirable to have it removed, as it disfigures the person affected with it. Almost every surgeon has a favourite plan of treating *nævus*, with the results of which he is well satisfied; but I doubt whether any other mode of removing some of these unsightly marks is as good as electrolysis, and those who have once used it will probably always use it again. Over excision, it has the advantage of being entirely bloodless; over the injection of perchloride of iron, that it is not dangerous to life; over cauterisation by nitric acid, that it can be better localised, and acts more thoroughly; over the subcutaneous ligature, that, once the operation has been performed, no further suffering is caused to the child, or trouble to the medical attendant; and over the galvanic cautery, that it does not leave a scar. It is rapidly successful in the flat round tumours of the size of a shilling, which are only slightly raised above the skin, but acts less quickly in extensive “port wine marks.” Where *nævus* assumes the form of large fleshy masses or lumps, the treatment is rather tedious, and, unless the patient perseveres, not so so satisfactory in its results. An ordinary round flat *nævus* yields to one electrolytic application, while in extensive “port wine marks” half a dozen or more applications may be required. The current should be directed to the tumour by means of fixed rows of gold needles, connected with both poles of ten to fifteen cells of Becker-Muirhead’s battery. As soon as the connection has been made, a destruction of the texture of the *nævus* is seen to commence, the blood-vessels and the skin withering away rapidly under the eyes of the operator. The destruction is more thorough at the positive than at the negative pole, and the worst parts of the *nævus* should, therefore, always undergo the influence of the former. As a rule, not a drop of blood is lost during the operation; but if, by sudden movements of the child, one of the needles should come out prematurely and a drop of blood should appear, this can be immediately coagulated, and any hemorrhage checked, by applying the positive pole to the puncture. The whole mass of the tumour is then gradually brought under the influence of the current, care being taken not to act too long or too energetically upon any one point, as otherwise a scar might be left. When everything morbid appears to have been destroyed, the

action is discontinued, and the surface covered with a piece of goldbeater's skin. There is no pain or discomfort after the operation; no dressing is required, for there is no discharge; the scab remains dry, and falls off in ten or fourteen days, leaving a healthy surface which gradually assumes the appearance of the surrounding skin.

2. *Bronchocele*.—Cystic bronchocele yields to a number of operative procedures, such as the seton, injection of iodine, or perchloride of iron, &c. Amongst these means, electrolysis may claim a foremost place from the comparatively painless nature of the proceeding, the absence of risk to life, and the circumstance that there is, as a rule, no open wound or sore produced which requires dressing. The best mode of proceeding is to insert two or three needles singly into the cyst, and connect them by means of the serres-fines conductor with the negative pole of the battery, while the positive electrode connected with a moistened sponge is placed on the skin in the neighbourhood. The consequence of this is, that the liquid in cyst is decomposed, the chloride of sodium solution being changed into one of caustic soda, whereby the secreting membrane of the cyst is cauterised and prevented from effusing further liquid. From two to six applications appear to be sufficient for a cure.

Solid Bronchocele is much more difficult to deal with, and the ablest surgeons of the present day habitually refuse to interfere in such cases, there being imminent risk to life attached to such operations as were previously practised. These tumours, however, often become dangerous to life by pressure on important neighbouring organs, and electrolysis then steps in as the right thing in the right place. I have of late years combined injections of the tincture or liniment of iodine into the substance of the tumour with the electrolytic applications, as it seemed to me that by such combined treatment time was gained. Iodine, subcutaneously injected, appears to break up the internal structure of the tumour, more especially where this is very old and tough, and aids electrolysis by allowing a freer passage of the current through the mass. In recent tumours, electrolysis alone is sufficient; but most cases which have been under my care have been of many years' standing.

I will now relate the particulars of a case of double solid bronchocele which I have treated in this manner, and which may be considered typical of this class of cases.

Case.—J. L., aged 37, single, a native of Yorkshire, and engaged in a mercantile firm in the city, came under my care in April 1874. He had for the last ten years been subject to a tumour in front of the left side of the neck, involving the thyroid body, which had very gradually increased until it

reached an inconvenient size. The patient had on various occasions consulted the heads of the surgical profession in London, with the view of having it removed. This, however, was not considered expedient by the eminent men whose opinions the patient had sought, and nothing had been done except painting the skin over the tumour with iodine, which had no beneficial effect. Things had gone on for a considerable time in this manner, when on April 10th, suddenly a fresh tumour appeared on the right side of the neck, over the collar-bone. This was accompanied by an increase of temperature in the parts, and the swelling showed considerable pulsation. At the same time, alarming symptoms of pressure on the pneumogastric nerve became apparent; viz., loss of voice and of the power of swallowing, a sense of choking in the throat, and severe pain at the back of the head, on the right side. There was regurgitation of liquids through the nose, and the patient had not slept for four days and nights, but was obliged to sit up in bed propped up by pillows, or to lean forward in a chair.

Under these circumstances, he had again consulted Sir William Fergusson, and entreated him to operate. Sir William, however, declined to do so, and the patient was then sent to me by his usual medical attendant, Dr. Black, of Islington. I first saw him on April 13th, 1874, and found him in the following state. The pulse was 120; respirations, 36; anxious expression of countenance; eyes staring and protruded; excessive action of the left ventricle. He had not been able to eat or sleep for four days, and could only talk in a whisper. In front of the left side of the neck, there was a tumour of the size of a small orange, which was very hard, and showed no signs of fluctuation. It was limited interiorly by the thyroid cartilage and windpipe, which were considerably displaced to the right side; exteriorly, by the strongly pulsating carotid artery; superiorly, by the horizontal branch of the lower jaw; and inferiorly, by the apex of the trigonum supraclaviculare. On the right side, there was a small but strongly pulsating tumour occupying the space of the supraclavicular triangle. It was much softer and less prominent than the one on the left side. The circumference of the neck on the most prominent point of the left side was $16\frac{1}{4}$ inches, and on the right side $15\frac{1}{2}$ inches.

The patient was in such a state of prostration, and so racked with pain and distress, that I thought it best to commence the treatment with an external application of the constant current, for the purpose of soothing and strengthening the central nervous system and diminishing the effects of pressure upon the pneumogastric. He recovered his voice almost immediately; had some sleep the night after, and no more regurgita-

16

tion of liquids. Next morning, the expression was less anxious; the pulse had gone down to 90 beats; respirations, 28; the voice went away and came again from time to time. On April 15th, he reported that he had had four hours' sleep; eating had become easier; he had drunk off a whole tumblerful of beef-tea without trouble, and the voice was stronger. The tumour on the right side was smaller, and its pulsation much diminished. The headache was still intense, and did not seem to be affected by the external application of the current. On April 19th, I introduced a needle connected with the cathode of fifteen cells of Becker-Muirhead's battery into the tumour on the right side, and allowed the current to pass through it for fifteen minutes. On the 21st, the patient reported that he had eaten some fowl and pudding, and that the headache was nearly gone. The old tumour on the left side was now likewise attacked by electrolysis and parenchymatous injections of iodine.

I will not weary your attention by further details about the treatment and progress of this case. Suffice it to say that by the middle of May the tumour on the right side had entirely disappeared, and that the left was then shrinking visibly under the influence of the treatment, which was continued, with some considerable intervals, until the middle of March last. At that time, there was only just a trace left to show that there had been once a tumour; the circumference of the neck was reduced from $16\frac{1}{4}$ inches to $13\frac{7}{8}$ inches. The patient has since resumed his work in the city, and was, when I last saw him (July 1875), in all respects in good health. Sir William Fergusson had an opportunity of examining him again about that time, and expressed to me his gratification at the successful result of my treatment in a case which he had thought beyond surgical interference.

It may be asked whether the result in this case was more owing to electrolysis or to the injections of iodine. Having carefully watched the effects of both, I have come to the conclusion that, of the two agents employed, electrolysis was by far the most effective. Indeed, one of the tumours yielded entirely to this alone in a comparatively short time. The iodine, however, unquestionably aided the action of the voltaic current in the manner described above; and I have since then, in view of this result, treated similar tumours again in a similar manner.—*British Medical Journal*, Nov. 13, 1875, p. 606.

ORGANS OF CIRCULATION.

45.—CASE OF AORTIC ANEURISM SUCCESSFULLY TREATED BY THE DISTAL LIGATURE.

By THOMAS ANNANDALE, Esq., Surgeon to the Edinburgh Infirmary, and Lecturer on Clinical Surgery.

R. B., aged 62, was first admitted under my care in August, 1874, on the recommendation of Dr. Wilson, of Motherwell. The patient had suffered from symptoms of thoracic aneurism for about six months; and, as these were steadily increasing in severity, he was advised to come into the infirmary.

On admission, there was a distinct aneurismal tumour, pulsating strongly, and passing up from behind the clavicle and sterno-clavicular articulation into the neck, as far as the cricoid cartilage. The trachea was displaced towards the left side by the tumour, and the inner half of the clavicle and its articulation with the sternum were pushed forward by the portion of the tumour under them. There was a well-marked *bruit* to be heard on all sides of the swelling, and there was dulness on percussion over a considerable area, corresponding to the situation of the thoracic part of the tumour. In addition to these symptoms, the patient had a constant irritating cough, pains shooting up into the head, want of sleep, and he was losing flesh markedly. He was ordered to keep in bed; and iodide of potassium, in twenty-grain doses, was given three times a day. After one month of this treatment, he left the hospital with his symptoms somewhat relieved, and he was advised to continue the iodide of potassium.

On February 8th, 1875, he was again admitted into my wards, as his symptoms had returned and become more troublesome. He had been unable to continue the iodide regularly, owing to the expense of it. An examination of the tumour showed an increase in its cervical portions. It had not only spread higher up, but it had also spread laterally so as to overlap the site of the subclavian artery. The upper portion of the tumour felt softer; and it was this portion which seemed to be increasing most rapidly. After carefully considering the circumstances of the case, and determining that pressure on the right carotid artery, immediately above the cervical portion of the tumour, very much diminished the aneurismal pulsation, it seemed to me a favourable opportunity for practising the distal ligature. The position of the tumour did not permit the subclavian artery to be ligatured; but there was just sufficient room to secure the common carotid above the cervical portion of the aneurism.

The patient having given his consent, I, on the 2nd of March ligatured the common carotid immediately under the omo-hyoid

muscle, which was drawn upwards, so as to reach the vessel. The internal jugular vein was unusually large and dilated; and some care was necessary to avoid injuring it, as it completely overlapped the artery. The operation was performed under the carbolic spray, and the ligature used was prepared catgut. The immediate effect of the ligature was to almost stop the aneurismal pulsation, and to convert the strong pulsation into a kind of quivering motion. Not the slightest local or constitutional disturbance followed the operation; and the wound was healed on the 17th. A week afterwards, the patient was out of bed. The day after the operation, the patient expressed himself as greatly relieved. He no longer himself felt the pulsation in the tumour, and the pains in his head and neck had disappeared. The pulsation in the aneurism was felt to be very feeble, and the tumour itself was decidedly smaller.

The patient left the hospital a few weeks afterwards, his symptoms continuing in the same improved condition, and the tumour gradually becoming firmer to the feel.

From time to time, the patient returned from the country to show himself, and his state on September 27th was as follows: His general health was good, and he had no pain or other uneasiness. He could stoop freely without giddiness, and could go up and down stairs easily. The tumour had continued to diminish in size, and its cervical portion was fully half an inch lower in level than before the operation. The whole tumour had become much flatter, and also firmer to the touch. On placing the hand over it, only a very feeble pulsation could be felt, but the pulsation was slightly stronger over the upper part of the cervical portion. A *bruit* could still be heard on all sides of the tumour, but it was not nearly so loud as formerly.

Remarks.—Although it cannot be said that the aneurism in the case reported was completely cured, there can be no doubt as to the great relief which followed the operation. The case, therefore, appears to me to furnish additional evidence in favour of the distal ligature in suitable examples of otherwise incurable aneurisms, and it also tends to confirm the opinions of Mr. Holmes and Mr. C. Heath, who have so ably written on the subject.

From my experience in other cases of thoracic and cervical aneurism, and from an observation of cases treated by my friend and colleague Dr. G. Balfour, I can testify to the value of iodide of potassium in relieving the symptoms and promoting coagulation in this disease; but I am inclined to express the opinion that, should a fair trial of this drug fail to give complete relief to the symptoms, the distal ligature of the carotid should be employed, provided the case be a suitable one for the operation. The test of the suitability of the case for operation being the

effect which temporary pressure on the distal portion of the carotid has on the aneurismal pulsation. If such pressure diminish the aneurismal pulsation, and should there be sufficient space to ligature the common carotid above the tumour, then I think the case is a proper one for operative interference. I also think with Mr. Holmes that operative interference should, in the first instance, be confined to ligature of the carotid, ligature of the third part of the subclavian being resorted to if advisable at a future stage in the progress of the case.

The case reported further illustrates the value of the antiseptic catgut ligature and other antiseptic precautions, the combined use of which has, in my opinion, removed all the most serious risks in connection with the ligature of arteries.—*British Med. Journal*, Oct. 30, 1875, p. 550.

46.—ON A RECENT CASE OF LIGATURE OF THE EXTERNAL ILIAC ARTERY BY AN ANTISEPTIC MATERIAL,
FOR THE CURE OF AN UNUSUAL NUMBER OF ANEURISMS.

By OLIVER PEMBERTON, Esq., Professor of Surgery in Queen's College, Birmingham.

In the number of the *Lancet* for Feb. 5th a brief note recorded the fact that nine days previously I had secured the external iliac artery in the case of a patient 47 years of age, in whom were present in the same limb three considerable aneurismal tumours—namely, one beneath Poupart's ligament, one at the apex of Scarpa's space, and one in the popliteal region. Beyond this it was noticed that a special animal ligature was employed, and that, in reference to the question of the renewal of the circulation in the limb, the collateral development, before it could succeed, must overcome no less than four absolute barriers established in the main trunk—namely, at the seat of ligature and at the situations of the three solid aneurisms respectively.

The lapse of three weeks since the performance of the operation enables me to speak with confidence on one or two points just now of great importance, more especially as regards the value of the antiseptic method of ligaturing an artery in its continuity.

The wound for the application of the ligature is soundly healed, having for the first fourteen days been thoroughly antiseptic, and for the subsequent ones being dressed with a resin ointment to remedy some adjacent excoriation. The aneurismal tumours are firm and contracting in dimensions. Gangrene ends by a well-defined line at the middle of the leg, the dead part being dry, absolutely without putrefaction, under an antiseptic dressing of cotton-wool prepared by saturation in a solu-

tion of carbolic acid in ether. The patient is well, and beyond suffering pain near the line of demarcation, is undisturbed by the death of his foot.

And now I come to the centre point of interest in this case, which is, I think, complete, and which circumstance alone is my reason for referring to it at this period, ere, indeed, the general result can be completely appreciated and recorded. That centre point is the having been able to tie a diseased artery so as to escape, by the mode of operation, the risk of secondary hemorrhage—a risk more than ordinarily threatening in the face of the existence of such extensive disease in the same arterial trunk.

I asked myself this question, Will the compressing power of the animal ligature I am about to use last sufficiently long to establish an adequate coagulum? If it will, I shall cure my patient's aneurisms. If not, I dare not tie the artery close to an aneurismal sac with a ligature that has to cut its way through the vessel.

I venture to think that a satisfactory answer is afforded by what has happened in this case to the several misgivings alluded to by Mr. Maunder—amongst others, as to the conduct of such a ligature as the one I employed. It will be for Professor Lister to say of what material the ligature he gave me was made up. Such a ligature as I used I feel assured will hold in any number of cases in which it may be tried; round, smooth, not thick, it was the perfection of a material, as it seemed to me, to make a firm and unyielding knot.

I have but to record, that with it I tied a large and probably diseased artery close to an aneurismal sac; that I tied it so as to be satisfied the outer coat was not severed, whilst I should think the internal ones were; that all suppuration was prevented about the tied artery by antiseptic dressing; that the wound was soundly healed at the end of three weeks, and that pulsation had ceased in the aneurisms from the moment of the operation being accomplished.—*Lancet*, Feb. 26, 1876, p. 310.

47.—TORSION OF ARTERIES.

By Dr. ROBERT M'DONNELL, F.R.S.

[It is unquestionably owing to the exertions of Mr. Bryant, of Guy's Hospital, and of Professor Humphrey, of Cambridge, that torsion has now become an established practice. Its latest rival is the use of carbolised catgut ligature for arteries of even the largest size.]

Any one desirous of making himself expert in the art of arresting bleeding by torsion should begin by making a few experimental trials on vessels taken from the subject. One or

two simple principles are thus learned. Suppose we take a few inches of the femoral or brachial artery cleanly dissected out from a recently dead subject. The end of the vessel is caught hold of by a pair of forceps (closing with a spring-catch or bolt) broad enough to catch the entire mouth of the vessel. By means of a second pair of forceps, the vessel is seized at right angles. This second pair of forceps consists of two blades; a projecting ridge on one blade fits into an indentation on the other. Seized transversely in this second pair of forceps, the vessel is squeezed tightly, and thus its internal tunics are nipped across, while its outer coat, which is much tougher, is not. The second pair of forceps have now done all that is required of them, and the twisting is accomplished by the first pair, the vessel being overlaid and twisted at the point where the inner tunics have been nipped through. It is curious and indeed interesting to observe what happens as the twisting is continued. The inner tunics actually peel off and double up, becoming invaginated in the vessel; the external tunic twists into a firm cord with little or no tendency to untwist. The appearance of this invagination of the inner tunics is well represented by Mr. Bryant in the illustrations accompanying his paper. It was, however, admirably figured long before by P. J. Manec in his works, "*Dela Ligature des Artères*," 1832 (plate xi., figs. 2, 3, 4, and 5.) A few trials, however, on the arteries of the dead subject, and a careful inspection of the result will do more than any illustration to satisfy the surgeon of the security which arises from the peculiar mode in which torsion obstructs the end of an artery. I would venture to assert that no surgeon who will take the trouble of investigating the matter by a few experiments of this kind will long resist the practice. It is obvious that torsion is not to be applied to all kinds of vessels in exactly the same manner. Experience teaches that we must adopt somewhat different expedients in securing by torsion vessels differing in size and position; the greater or less laxity of the tissue which surrounds the vessel, the character of its sheath, &c., all make it necessary to modify somewhat the mode of procedure. The surgeon should have at hand, therefore, several varieties of torsion forceps, since a pair well suited to twist one vessel may not answer so well for another. Perhaps the simplest and most practical mode in which I can hope to convey to those who have not as yet learnt the mode of torsion for themselves some idea of the details of the practice is by describing as accurately as I can how it may be done on different vessels. *First*, then, vessels met with in amputations of about the size of the radial or ulnar are easily secured. They are best twisted by the aid of toothed forceps. These forceps are, I believe, generally known as "*Syme's torsion forceps*." The

vessel is to be seized right across its open mouth and drawn out a little ; it is then caught just beyond the forceps, between the thumb and forefinger of the left hand, so that the nail of the thumb may give it a good pinch, and nip through the internal tunics ; then twist and all is secure. The end may in the case of such vessels be twisted off or not, as the operator pleases. If the forceps catch the vessel by the lip only, or at the side, it will of course continue to bleed. In order to be successful the vessel must be caught entirely across the mouth, and the outer tunic must be twisted. *Secondly*, if the vessel be surrounded by a large quantity of lax areolar tissue, as in the scrotum, prepuce, around the eyelids, or in the loose tissue forming the bed from which a fibrous tumour has been removed, the forceps last mentioned do not answer well. The teeth project too much. When twisted they catch in the loose areolar tissue, as a corkscrew would if twisted in tow ; this prevents the proper twisting of the vessel itself at one point, viz., where the inner tunics are divided by being nipped. For vessels of this sort such forceps as are here (*forceps exhibited*) are very good. As before, the vessel is to be caught across the mouth, if possible, nipped by the nail of the thumb of the left hand, and twisted so that the external tunic is overlaid at the point where the nip has been made. *Thirdly*. Vessels lying in muscular tissue are sometimes troublesome to secure by twisting. This is not so when, as on the face of a stump, the vessel can be fairly and completely caught by the mouth. Here muscular branches of an artery are as readily secured as other vessels. But suppose, for example, that a breast has been removed, and the great pectoral exposed ; a bleeding point on the muscle is seized and twisted ; the part seized tears away readily and leaves a little pit bleeding more than before. The vessel has in truth been caught sideways and a piece torn out of it, all the more readily because the muscular tissue in which it is imbedded is friable. The difficulty in these cases arises from the fact that where we have to deal with a longitudinal and not a transverse section of a muscle there is more trouble in making sure that we catch the vessel right across the mouth. If it be scraped with the handle of a scalpel, perhaps, if need be, rather roughly, so as to let the vessel be distinctly seen, and caught, not longitudinally, but transversely across the mouth, the twisting may be performed as readily as on any other vessel. *Fourthly*. Some special difficulty may be experienced in securing by torsion vessels which lie deeply between bones and in the close vicinity of a dense membrane,—as the interosseus arteries in the forearm and leg. In this case the vessel must be detached carefully, either by the handle of the scalpel or by dissection from its surroundings. Obviously if, in seizing such a vessel, the interosseus membrane is also

taken hold of by the forceps, a successful twisting would be impossible. From this membrane the vessel must be entirely separated. *Fifthly*. Such vessels as the brachial, femoral, or popliteal, are in general very readily secured by torsion; in the great majority of instances it is only necessary to attend to the principles already laid down, viz.: 1st, to catch the vessel clean across the mouth; and 2nd, to divide by some means the inner tunics and twist the outer coat at that point. In all cases of amputation for injuries, where the tissue round the vessel is healthy, it is usually quite an easy matter so far to detach from the adjacent parts as to accomplish in a few seconds all that is necessary. If, however, we are amputating in the neighbourhood of diseased structures, sinues, &c., where the parts are matted together, it is necessary by some touches of the point of a scalpel to free the vessel from the surrounding parts. Even then it cannot be drawn out as in the former case. The parts around are unyielding, and it is by no means easy to get space enough beyond the ends of the first or grasping forceps to apply the second or nipping forceps. Under these circumstances I adopt the following expedient. I place a silk ligature round the vessel immediately beyond the ends of the grasping forceps (which for large vessels are to be made big enough to catch the entire mouth of the artery). This ligature I pull home until it snaps across. To make sure of accomplishing this with some facility I have the silk fixed on wooden cylinders, so that it can without hurting one's fingers be pulled home until it snaps across. It invariably snaps at the knot, and is to be pulled off and cast aside. We thus accomplish, by a snapped ligature, what is done in other cases by the nipping forceps, and then proceed to twist; three or more half-turns are sufficient for the femoral, and I always in this case leave the end still adhering. If the wound is washed with chloride of zinc solution or otherwise properly antiseptized with carbolic acid water, &c., these portions do not act as foreign bodies, but are absorbed as Lister's catgut ligature is.

[Dr. M'Donnell then exhibited the brachial artery of a man who had died five days after an amputation of the arm, in which the brachial artery had been twisted. He said the vessel was perfectly secured, and that the extreme end which had not been twisted off showed no disposition to slough. This case was sufficient to prove that the objection raised as to torsion—that the extremity of the vessel was likely to slough and act as a foreign body—was groundless. There was in truth no more likelihood of this occurring after torsion than after the application of an ordinary ligature, as in this case also the extremity of the vessel had the same risk of sloughing.]

Dr. M'Donnell then proceeded;—The mode which I now

propose, and which I have over and over again put in practice, of using a silk ligature, not too strong, pulled home until it breaks, for the purpose of dividing the inner tunics of the vessel previous to twisting, is very simple, and, I may add, very effectual. It supplies the place of the nipping forceps or the thumb-nail, and is applicable to vessels of all sizes which are unmanageable in other ways. In fact, no readier way of securing ordinary sized arteries can be suggested than to put on, first, a silk ligature, and then seize the knot in a pair of torsion forceps and twist it off; the vessel is perfectly secured, and there is no foreign body left behind in the wound. Large wounds are often then found healed on the twelfth to the fourteenth day; that is about the time that, in cases where a bundle of ligatures have been left in the wound, these are beginning to separate. Some ingenious instruments have been devised for performing with a single instrument the double manœuvre of nipping the coats and grasping the vessel to twist it. Among these I would mention a kind of forceps invented by Dr. Addinell Hewson, surgeon to the Pennsylvania Hospital in Philadelphia. Dr. Hewson was good enough to present me with a pair of these forceps last year, when I had the pleasure of visiting his city. I have since then had several opportunities of using them, and found them do their work satisfactorily on vessels not surrounded with brawny, tough, infiltrated structure. Where, however, this condition exists, or, in fact, wherever there is any difficulty in drawing the vessels out a little from the surrounding structures, Dr. Hewson's forceps are open to the same objection as arises in attempting to use the cross-nipping forceps. There is not space enough to apply them, and to nip properly beyond the ends of the grasping forceps. The advantage of the ligature pulled until it snaps and comes away is simply that we can in this way accomplish all we wish to do as regards dividing the inner coats upon a very limited piece of the artery.

[Dr. M'Donnell also exhibited a pair of forceps devised by himself, and made by Messrs. Weiss and Co., of London, and designed also by a double manœuvre to nip through the inner coats and seize the mouth of the vessel so as to twist it; but he said he had not found the instrument successful in actual practice.]—*Medical Press and Circular*, Feb. 23, 1876, p. 153.

48.—THE TREATMENT OF NÆVUS.

By Dr. JOHN DUNCAN, M.A., Assistant-Surgeon, Royal Infirmary, Edinburgh.

For practical purposes, nævi may be divided into two classes—those which are stationary, and those which are spreading.

Of the former, so large a number disappear, and the cure by nature is so much safer and better than the cure by art, that the first proposition which I have formulated admits of few exceptions. Two exceptions may be made, viz. (1) when the nævus is exceedingly small; and (2) when the means of cure are absolutely innocuous.

Nævi about the size of a pin's head, and seated on the skin, are not uncommon on the face. They sometimes spread, they are very enduring, and it is therefore well to destroy them while yet small. The best method is to puncture the little spot with a needle previously dipped in strong nitric acid. This is simple, certain, and leaves no visible mark. It may be destroyed with equal certainty by the heated needle, by the galvanic cautery, or by electrolysis; but these measures are inferior in simplicity and are prone to leave a more distinct cicatrix.

Of all the modes of treatment which have been proposed, pressure is probably the only one which can be called absolutely innocuous. Unfortunately, it seems equally innocuous to the nævus and the patient. Even its advocates demand a suspiciously long time to effect a cure thereby. It is evidently only in some situations that pressure can be continuously kept up; and in these the prolonged annoyance from the use of the necessary apparatus outweighs the slight chance of cure. I have sometimes, when a nævus of small size is situated over a bone, as on the forehead, advised the application of collodion every second or third day. A considerable number gradually disappear during the process; but I have not satisfied myself that the disappearance is its effect. It has, perhaps, the merit of keeping the patient's friends for a time harmlessly amused.

From this verdict of non-interference in stationary nævi, yet another exception must of course be made. I allude to those which persist beyond the age of puberty. The hope of a natural cure is then gone, and the deformity which they produce thus becomes a justification for treatment. They may be referred to the same category and be subjected to the same remedies as the spreading nævi which we have now to consider.

In this, the second class into which I have divided nævi, it is perfectly justifiable to interfere. Extreme slowness, or extreme rapidity of increase, may sometimes warrant delay. Thus, if the growth be very slow, and getting slower, it may be right to await a possible retrogression for a limited time. In other cases, from the very rapidity of growth, it is difficult to see how any interference can be effectual. I have watched a case in which, at birth, the only trace of nævus was a small mark on the temple, supposed to have been produced by a bruise in

delivery. Within a fortnight, little patches of nævoid skin were observed over the side of the forehead, head, cheek, and palate, which speedily coalesced and formed a prominent mass, closing the eye, pushing forward the ear, and greatly enlarging the upper lip. It was impossible to contend with the growth; but it is satisfactory to be able to add, that now, three years afterwards, it has spontaneously almost disappeared.

With these exceptions, however, I believe that we ought to attack every spreading nævus. To this variety are due what risks in the way of hemorrhage and ulceration may attend the disease; and while, no doubt, the majority would, if left alone, end by disappearing, yet, in the remainder, the operation which has ultimately to be performed becomes more difficult and dangerous exactly in proportion to the size of the growth. At the same time it must, as already said, be borne in mind in choosing measures, that our justification in operating is the deformity and inconvenience, rather than the danger of the disease; and that, although these may warrant us in running a moderate risk, it is a strong argument against seriously hazarding the life of the patient. The same considerations ought also to make us careful not to undertake an operation, of which the result is more unsightly than the disease; nor, in eradicating swelling and discolouration, to leave a face slashed with scars, "as if it had been operated upon for double harelip."

The means at our disposal may be divided into two classes—(1) those by which a scar is avoided; and (2) those which necessarily leave a cicatrix of greater or less magnitude. If these were of equal value in other respects, it is plain that the former ought always to be preferred. But, unfortunately, this is not the case. Those which involve destruction or division of the skin have the advantage of greater certainty and rapidity in producing a cure; and although the subcutaneous methods of treatment are in most cases less dangerous, this quality is not sufficiently pronounced to render them preferable when a scar may be unimportant. I think a general rule may be laid down that in operations on the face and other exposed portions of the body, the first class ought to be used, because they avoid disfigurement, and because, although a single operation may not suffice, repetitions in the end succeed; but that, on the other hand, the more rapid and certain methods should be applied elsewhere.

The first class aim at the destruction of the texture of the nævus, or the coagulation of the blood which it contains, by agents which can be introduced through minute punctures of the skin. The means used are the injection of coagulating fluids, the introduction of setons, the subcutaneous ligature, electrolysis, and the galvanic cautery.

Many different fluids have been used as coagulants. They all have a double action, in securing coagulation, and in setting up an inflammatory action, which tends to obliteration of those vessels which may have escaped their direct effect. The most reliable appear to be perchloride of iron and carbolic acid. Their special advantages are that they are powerful coagulants, and that while their caustic properties excite inflammation, this inflammation has little tendency to pass beyond the adhesive type. One danger attends them which limits their applicability. A considerable number of cases have been recorded in which a rapidly fatal issue has followed the injection. This appears to have been due to the escape of the fluids or clots beyond the limits of the nœvus, and consequent coagulation of the blood in parts essential to life. At all events, it is certain that, unless the circulation through the nœvus can be thoroughly controlled by temporary ligature or otherwise, injection ought not to be performed. Another danger, or rather difficulty, also deserves attention. It is by no means easy so to apportion the amount of injected material to the size and viability of the tumour that, on the one hand, the action may be sufficient to obliterate the vessels; and, on the other, not so severe as to cause the whole mass to slough off. I have seen in careful hands, in one instance, the upper lip destroyed; and, in another, ectropium produced by destruction of skin in the eyelid. Care should be taken that the fluid injected be distributed over the growth in minute portions; and the surgeon should be careful to do less at one sitting than is necessary for obliteration, trusting rather to repetitions of the operation, which ought not to be made at too short an interval. I believe that a preliminary disruption of the texture of the nœvus with a tenotomy knife is advantageous, by permitting greater diffusion of the coagulating fluid, and therefore a greater effect with a smaller quantity than otherwise would be the case. With these precautions, that the circulation be controlled, and the amount injected kept within due proportions, I believe this method of treatment to be the best we have for nœvi of moderate size situated on the face, if they be mainly subcutaneous. It is safe, very successful, and leaves no scar.

The passage of setons, and the subcutaneous ligature, are remedies allied to each other in their effects and mode of action. They obliterate vessels, excite inflammation, and bring to bear on the growth the contractile effect of a scar. Setons, whether simple or steeped in an irritating and coagulating fluid, appear to offer no advantages over, and in some respects are decidedly inferior to, the subcutaneous ligature. They are inferior in the large number of suppurating orifices, and consequent cicatricial marks which they produce. The subcutaneous ligature, more-

over, has the advantage of at the same time exciting inflammatory action, and cutting off a large part of the vascular supply. It may be made of various materials—cord, silk, or wire. I have lately used the elastic ligature with good effect. To establish it in position, a strong cord must first be passed round the growth subcutaneously. This can be easily done in the largest nævus by bringing out the needle once, or at most twice, in the course of its passage round the circumference. But if it be intended to draw through an elastic ligature by means of the cord, the needle in a large nævus should be brought out three, or even four times, that too great a strain may not be made on the elastic. Lest even then the elastic ligature should break during traction, I have always left the cord long enough to be attached to another. When the ligature has been established, the cord may of course be withdrawn. The advantages of the elastic material are the continuous tension which it maintains, and the ease with which the tension can at any time be increased. In order to prevent the knot being drawn under the skin, I use a metallic button with slits, into which the ligature passes when stretched, and which accordingly holds it when relaxed. To render it still more secure, the ends may be tied together over the button.

The method of subcutaneous ligature is not without its drawbacks. It necessarily produces a certain amount of suppuration, and in some cases this is accompanied by considerable constitutional irritation. I had lately a large nævus of the back sent to me by Dr. Macnee of Inverness. It measured 4 by $3\frac{1}{2}$ inches, and was of great thickness. Dr. Macnee had already used setons, but although for a time they appeared to do good, ultimately the nævus increased again with great rapidity. I established an elastic ligature which came away in ten days. During that time the child (a few months old) was restless, feverish, and had a good deal of diarrhoea, as had also been the case when setons were used. The nævus shrivelled and diminished greatly, but I have since heard that it has not disappeared. I mention this case as the most unfavourable I have had, because it shows that the result of the subcutaneous ligature is not absolutely certain, and that sometimes it causes anxiety from constitutional irritation—an effect which we might expect from the analogy of teething. I am of opinion, however, that these large nævi are best treated in this way. Excision would be a severe operation in so young a child, and the raw surface left by external ligature would be very great. On the other hand, had the case I have mentioned, in which I was warned of the constitutional irritability, come before me now, I should have proceeded by the next method to which I have to refer—electrolysis, as being, although tedious, not prone to cause constitutional disturbances.

Electrolysis may be performed in two ways, viz., with and without insulation of the electrodes. When the needles are not insulated, the operation consists simply in causing more or less complete sloughing of the nævus. The sloughs are precisely similar to those made by caustic acids, if the positive pole be used mainly or alone; to those made by caustic alkalies, if, as is better, the use of the negative predominate. This method, therefore, is one that leaves a scar, and offers no advantage over destruction of the growth by caustic, except, perhaps, in being somewhat less painful; no advantage over ablation, except that the scar is somewhat smaller. I have used the method, when the skin was largely involved, by passing fine needles under and parallel to the skin. It occasionally does good, especially when combined with other measures; but really no treatment, except entire destruction of the skin, is thoroughly reliable in these cutaneous stains. Electrolysis with uninsulated needles has a very limited application. Electrolysis by means of insulated needles might be regarded as our best method of curing nævi, but that it is more tedious than injection, less certain than ligature. The needle is insulated by means of vulcanite. The exposed point is made to correspond to the size of the nævus, and care is taken to bury it completely in the growth. The strength of current may vary between three and twelve cells of Weiss's Smee's battery, according to the distance between the poles, and whether or not anæsthetic be used; and the duration of the action is determined by the feeling of hardness produced. Both poles should be inserted, because the current is then more effective, *i.e.*, the resistance is diminished, and there are two points of action. The number of needles will vary with the size of the tumour. I have already, in a former paper, related a number of successful cases. I would now merely summarise the conclusions of a fuller experience.

1. In a large number of operations, I have never met with an untoward result. In two cases of nævi situated on the head, I have observed alarming pallor come on suddenly during the operation, but I have been inclined to attribute this to an overdose of the anæsthetic, because in the same patients subsequent operations were performed without this disagreeable concomitant. There is a tendency to administer an overdose, for the patient, when apparently fully anæsthetized, is often roused by the closure of the circle, and more chloroform is given to quiet him. I may remark, that it is generally necessary to anæsthetize. The operations are prolonged and painful.

2. With moderate care, sloughing can be avoided, and supuration ought never to be seen.

3. With perseverance a cure may always be effected.

4. But, on the other hand, electrolysis by this method is extremely tedious and troublesome. I find from experience that no advantage is gained by repeating the operation oftener than once in three or four weeks, and that in a nævus of moderate size five or six operations are required.

5. I have already referred to the difficulty of curing a nævoid discoloration of the skin. The remark applies to the treatment by electrolysis, as well as to all other methods. A case sent me by Dr. Williamson, of Leith, illustrates this. The entire parotid region, auricle, and upper part of the neck on the left side, were occupied by an ill-defined, large, and prominent nævus, over which the skin was everywhere discoloured. By electrolysis the parts were rendered absolutely flat; but the skin became white only in the centre over a space the size of a crown, although, in this instance, I tried what in others I have found advantageous, the painting of the surface with nitric acid, blistering fluid, iodine, and other irritants.

There is yet another mode of using electricity, which I believe will come into more general use,—the galvanic cautery. It may be used subcutaneously with perfect ease. The platinum wire is placed in position in the growth, and a hollow needle is threaded on each end, pushed along it through the skin, and then connected with the battery. The motions imparted to the platinum wire will of course determine the amount of tissue cauterised. The contractile properties of the scar of a burn render it unnecessary to produce extensive destruction, and it would be advisable rather to trust to a repetition than run the risk of producing a slough. I have applied the plan only once to a nævus about the size of a pigeon's egg on the shoulder, and although I made a very limited use of the cautery, the growth afterwards gradually withered. I believe that, but for the cumbrous and costly apparatus required, it would supplant the seton and subcutaneous ligature, because it is not attended by suppuration.

There are three methods of treating nævi which necessarily leave a scar—excision, ligature, and ablation. They have this in common, that the cure is certain.

The danger of bleeding when excision is performed was at one time much exaggerated. It is now well known that in almost every case nævi are encapsuled. The capsule is doubtless formed, as in other tumours, by the pushing aside of surrounding textures during the growth of the adventitious structure. But, as in other tumours even of simple character, a loose capsule cannot be relied upon. Under pressure or other form of irritation the distinction between the growth and its surroundings is lost in adhesions. This gives rise to a

difficulty in dissection, which is usually of little moment, but which is a source of danger in the neighbourhood of important parts. Mr. Teale's operations, doubtless, show the possibility of excision in most unpromising situations, but such operations are more formidable than the disease warrants, and in many respects are inferior to other measures. Excision is inapplicable in the face, because it leaves a scar, and because it does not cure with certainty the nævoid skin. It has disadvantages in the body, because there is a certain amount of bleeding at the time, and possibly some risk of reactionary and secondary hemorrhage. It ought, I think, to be restricted to nævi which are subcutaneous, which are situated on a part of the body usually covered, and which are not of large size. For these, it is rapid, clean, and certain.

Ligature, on account of its safety and certainty, is justly regarded as the proper mode of treatment for nævi not on the face, and which can be completely encircled by the cord. The exceptions are those just mentioned as suitable for excision, and large nævi in which the destruction of skin would be excessive, and the scar would injure the usefulness of surrounding parts. I do not think that ablation presents any advantage over ligature. The scar is equally large, the healing is not more rapid, and the security from hemorrhage is not so great. Still, if it be at any time necessary to remove a nævus of the tongue or inner surface of the mouth, ablation by the galvanic cautery is plainly the best procedure. In such situations, methods which depend for their efficacy on inflammation or suppuration are inapplicable, and the presence of a ligatured mass which could not be prevented from putrefying, would be singularly offensive and even injurious.

I have not thought it necessary to detail, merely for the purpose of condemning, the thousand-and-one remedies which have been applied to nævus. Besides those I have mentioned, I do not think there are any of practical importance. We may have to vary our treatment according to the circumstances of the case, but our choice is really limited to a few measures, of which ligature, injection, and galvanism are the most prominent.—*Edinburgh Medical Journal*, Feb. 1876, p. 703.

ALIMENTARY CANAL.

49.—ON CLEFT PALATE.

By Dr. E. A. RAWSON.

Finding the usual means for approximating the edges of a cleft palate tedious, to say nothing of their difficulty, I have devised a little instrument which I believe will facilitate the

operation very much. It was made for me by Weiss some months ago, and since then I have not had the good fortune of a case to try it on. I therefore venture to suggest my plan to the profession in general, and trust that any one approving of it will try it and publish the result. The instrument is simply a safety-pin, or rather needle, used on the same principle as that for fastening children's clothes.

It is about three-quarters of an inch long and slightly curved: at its head is a flat silver knob, to which is attached a spiral silver spring; this spring is shorter than the needle, and on stretching it a sheath at its free extremity receives the point of the needle.

The edges of the cleft having been pared, the needle is passed in through one and out through the other, giving a good grip to secure apposition, and going nearly or quite through to the back; the silver sheath fastened to the spiral spring is then drawn across and receives the point of the needle, thus retaining the latter in its place. Two, or as many more needles as are required, can be applied, and when union is complete, can be withdrawn by pulling the sheath from the point of the needle and taking the latter out in the usual manner.

Of course all this must be done by means of handy forceps.

The pared edges of the cleft might also be held in apposition until union had taken place by means of a small trout-hook, fastened to each end of a spiral silver spring half-an-inch long; india-rubber, I fear, would soften; one hook held in a forceps to be plunged into one lip of the cleft, the other hook to be then seized and drawn across, extending the spring, and secured in the opposite lip; the barbs, I think, might be left on the hooks, as they would make certain of retaining them in their places, and every angler knows how to withdraw a *small* barbed hook—viz., by making a slight nick in the flesh at the back of the barb, pressing the hook slightly onwards, then outwards into the nick, when it will easily withdraw. This experiment can be tried on a piece of cloth.

Or, instead of a spiral spring, the hooks might be fastened to a piece of silver wire, which, when they were applied, could be twisted until the pared edges were in apposition.

I would recommend the very smallest trout-hooks, as larger ones might pierce through into the posterior nares, and when the soft palate was raised seize the opposite lining membrane. I can see an objection to the hooks, for in using them the back of the wound would not be in such close approximation as the front, but they would be much easier of application than the safety-needle, which, however, has the advantage that it would retain both back and front of the wound in perfect apposition.

In one case, that of a child, $2\frac{1}{2}$ years old, in which half-an-inch of the bony palate was cleft, I used the actual cautery as above, afterwards stitching with three wire sutures, which, however, all gave way. Sometime afterwards I received a letter from the mother to say that the cleft had healed, leaving a white seam in its place, and that the child could speak quite plainly. I went to see it, and found the cleft *partially* healed and the remaining part in such close approximation as to deceive a superficial observer.

Vesico-vaginal fistulæ and such like deep and difficultly situated lesions might be found easier of treatment by the foregoing.—*Medical Press and Circular*, Feb. 2, 1876, p. 88.

50.—ON HARE-LIP AND CLEFT PALATE.

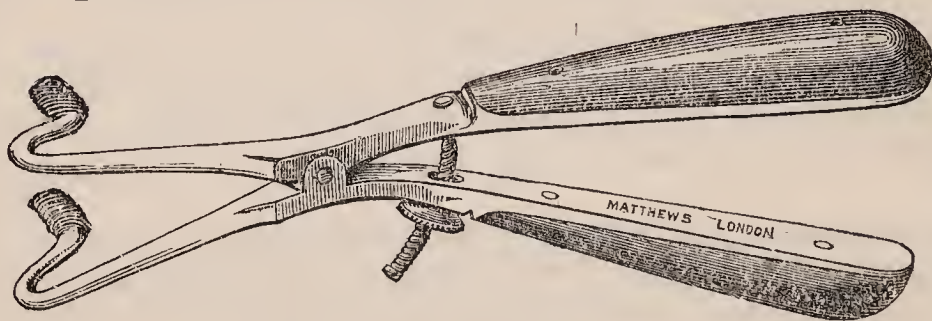
By Sir WILLIAM FERGUSSON, Bart., F.R.S., Surgeon to King's College Hospital, &c.

[In April, 1874, Sir William Fergusson had only had experience of his new operation for the cure of cleft palate in four cases (*Retrospect*, Jan. to June, 1874, p. 217). The number in which he now has had similar experience amounts to eighty-two.]

Experience has led me to make some modifications in practice formerly described, which I fancy worthy of the object of this present paper. In dealing with a cleft in soft and hard palate, I still adhere to my preliminary division of the levator palati on each side; but if from the size of the gap in the hard palate it be deemed needful to introduce one, two, or more stitches, I advise that the holes for them be bored with the bradawl, and that the stitches be introduced before the chisel is applied. I think it an advantage, too, to make the edges of the gap bare before loosening the portions of the palate with the chisel. If these parts be made loose first, it is very difficult to push a bradawl or needle through, and there is great risk of one or both slipping, so as to separate periosteum from bone and endanger the occurrence of necrosis. This I believe to have been the cause of the misfortune in the two cases above referred to, when the operations signally failed. But I fancy I have further modifications and improvement to impart here. Recent experience has led me to the fact that stitches may not be necessary in many instances of cleft in the hard palate. I have found again and again that, when the edges of the gap have been pared and the chisel introduced, it answers to cause approximation of the raw margins by stopping the opening made on the hard substance by the chisel, with lint, so as to make the desired closure in the centre. The pledgets of lint keep the parts as steady as, if not more so, than the stitches,

whilst they obviate the necessity of the additional injury of the stitch-punctures. This practice, I am confident, is available in all instances when the opening in the bones is of brief extent, as the front stitch in the soft palate may be put in close behind the posterior margin of the bones.

In my former paper I referred to gags, and particularly to those of Mr. T. Smith and Mr. Wood. For various reasons, which need not be dwelt on here, I have set these aside in favour of a more simple apparatus, chiefly suggested by my friend Mr. Mason, of St. Thomas's, and in a manner perfected by Messrs. Matthews, instrument-makers in Carey-street, which I strongly recommend as the best I have yet seen for the operation in question. In fact, I consider the gag as an essential instrument for the modern proceeding, for without it chloroform would be worthless. This sketch shows the instrument in question.



The narrow blades are introduced shut between the teeth on either side the surgeon chooses, and are opened by pressure on the handles at the will of the assistant who holds them. The power of the instrument is such that, if care be not taken, the lower jaw may actually be luxated, as I have seen more than once, although happily with no permanent evil. There is a peculiar twist in the blades of this instrument as regards the cheek and margin of the mouth, which can be better appreciated by looking at them in action, than from any description that might be given here. The length of the whole instrument is about seven inches, and it will be useful to have two, one slighter than the other, to suit the mouths of patients from four to ten years of age. This gag will be found of much value in many operations about the mouth and tongue, and I presume to say that it will prove a valuable aid to the dentist.

On thinking over all that I know of these matters, and especially as regards my own humble work, I am impressed with the slowness with which facts and improvements have come upon my mental conception. I believe that I was the first who dissected and described the anatomy and physiology of cleft palate (cleft, be it observed), yet five years or more passed

before the simple mechanical value of the information thereby obtained dawned on my mind. Neither the action of the levators in tugging upon the stitches, nor that of the superior constrictors of the palate in closing the cleft during deglutition, were thought of; but happily, when tenotomy and myotomy were in the full swing of novelty and fashion, I bethought me of my old dissection, which, having been fortunately preserved as unique, gave me the opportunity of further study and reflection. Yet, even now, after a comparative calm in my own mind, I am amazed at the slowness with which point after point has gradually come upon me in an experience of between three and four hundred operations, including old fashioned and new. These latter observations have been more impressed on my mind within the last two years than at any previous date.—*British Medical Journal*, Jan. 1, 1876, p. 3.

51.—THE USE OF LIQUOR BISMUTHI FOR HEMORRHOIDS AND PROLAPSUS ANI.

By Dr. JOHN CLELAND, F.R.S., Professor of Anatomy and Physiology, Queen's College, Galway.

[It is evident that in a large number of cases of hemorrhoids operative interference is inevitable, but it is desirable to avoid so disagreeable a mode of treatment as much as possible. With this end he recommends the use of liquor bismuthi as an enema.]

My attention was first drawn to this remedy by a rather peculiar case of prolapsus of the bowel. A middle-aged woman came for consultation in such a condition that she could with difficulty walk, inasmuch as whenever she parted her thighs, the bowel emerged and hung down for about six inches, in folds of such a character as made it evident that at least half a yard of intestine was extruded. The whole surface of the mucous membrane exposed was a deep raspberry red, like those cases of hemorrhoids which some practitioners delight to treat with nitric acid. This condition was chronic; external supports had failed; the possibility of removal of the whole prolapsed mass suggested itself, but such an operation attended with enormous risk was not to be thought of in the case of a patient enjoying a certain measure of health. Astringents had been tried and failed, and it seemed questionable if astringents were the most suitable remedies in such a case. It appeared much more probable that an irritated and congested condition of the mucous membrane led to a derangement of the action of the muscular walls than that in a strong woman, a local relaxation,

involving sphincters and intestinal walls, had produced a prolapsus, which led to congested mucous membrane, from exposure. I recollected the relief frequently obtainable in cases of hemorrhoids, by application of white bismuth or oxide of zinc. In this case, however, ointment or powder obviously could not be effectually applied. But the liquor bismuthi in stomach affections has a soothing influence far superior to white bismuth. I therefore directed my patient to mix a dessert spoonful of liquor bismuthi with half a wine glassful of starch, and after getting into bed and returning the bowel to its place, to introduce this enema and retain it. I was much pleased, a few weeks afterwards, by my patient calling to tell me that she was nearly well, and to ask if she might continue the remedy. This she was ordered to do; and I have every reason to believe that she has had no return of her malady. I have since frequently used the same remedy for the ordinary prolapsus in children, with invariable and rapid success.

In severe hemorrhoids there are usually three parts affected, the integument, the mucous membrane, and the hemorrhoidal veins. Plainly the veins cannot be reached by local medicaments, and those comparatively few cases in which they alone are involved must be treated in other ways. The integument, together with the edge of the mucous membrane up to the grasp of the sphincter, is within easy reach, and may be treated in various ways according to circumstances. Thus, when the congestion is superficial and produces a catarrhal oozing, bathing with whisky or other alcoholic lotion, a small pad of dry cotton-wool firmly applied to soak up the moisture, and also zinc or bismuth in powder or ointment are all exceedingly useful; and when a congested surface within easy reach is accompanied with venous engorgement, tincture of iodine sometimes produces surprising effects, although in other instances it is too painful to be borne. But when the mucous membrane is considerably involved, I know no application to compare with injection of liquor bismuthi, which has the advantage of being painless; and, as in the case of prolapsus narrated above, the improvement of the mucous membrane has a wonderful influence on both the veins and integument. In instances in which the necessity for surgical interference appeared indubitable, I have had the gratification of defrauding myself of the pleasure of operating, and of seeing the patient recover. This is the more gratifying, as the surgical treatment of hemorrhoids labours under the disadvantage, that, no matter what be the particular operation adopted, it never removes the predisposing cause of the malady.—*Practitioner*, Jan. 1876, p. 54.

ORGANS OF URINE AND GENERATION.

52.—ON THE TREATMENT OF URETHRAL DISEASE.

By Sir HENRY THOMPSON, Bart., Surgeon Extraordinary to H.M. the King of the Belgians; Emeritus Professor of Clinical Surgery in University College Hospital.

The treatment of urethral disease of which I complain has its origin in the notion that the urethra is a mere flexible tube, closed at or near its junction with the bladder by some kind of muscular apparatus, sphincteric or otherwise, through which fluids will pass indifferently in either direction. No idea, however, can be more erroneous, and treatment founded on it must be defective.

At the outset, then, let me say that it is absolutely essential that you should have a tolerably accurate knowledge of the nature and functions of this so-called tube.

The urethra is not a tube at all, in any sense in which we employ that word. It is not like a gas-pipe, or an india-rubber tube, or even a flaccid tube of any membrane whatever.

It is rather *a continuous closed valve, capable of transmitting fluids and solids in one direction only, and transmitting nothing whatever in the opposite direction, except in obedience to applied force.* Its length in the male makes us think of it as a tube, but this is a mere accident of sex. An inch or less is amply long enough for its urinary function, as in the female; and all the length it possesses above that is quite useless *as a urethra*, and renders it liable to disease and accident—the price, and a heavy one, let me tell you, which the male pays for his specially distinguishing feature. In illustration of this, I have but to refer you to the innumerable difficulties and dangers associated with stricture, retention of urine, and calculus, which are almost unknown in the other sex. It is, then, in the male simply a long valvular chink, traversing soft and most delicate vascular and nervous tissues, always firmly closed, and never opening except for a few seconds, during which fluids have to be transmitted from the body. Then, for a few seconds, it is distended more or less, and becomes a tube if you please, for this short time and this only, equalling, perhaps, at most three minutes in the twenty-four hours. All the rest of the time it is firmly closed, and not one drop of fluid can pass from the bladder. Of course, oozing of liquid which is generated in the walls of the tube, or which enters it by ducts may escape, but always, inevitably, in the outward direction only.

Now, during these few seconds, when the valve may be said to occupy the form of a tube, I have next to observe that it is

a tube of very varying diameter in its different parts ; it is, in fact, very differently distensible at different places, being surrounded by different structures. This fact has long been known and generally recognised.

Having thus far illustrated briefly, and necessarily somewhat imperfectly, the nature of this valvular passage, let us see how far the ideas which I want you to acquire relative to the urethra affect two important points in practice. First, that simple matter of making an injection into the urethra. You have to introduce a fluid for the purpose of therapeutic contact with the walls of this closed passage ; you have to distend it, and some little force is necessary ; not a single drop can enter, much less run down into it, unless the liquid is forced in by a piston, while the orifice of the urethra is carefully closed around the tube of the syringe introduced. The walls of the passage lying closely applied to each other become opened only by the pressure of the fluid driven in, and they are distended just so much and so far as the quantity employed determines. Thus you may safely reckon, as the result of my observation, that a syringe containing one fluid drachm is amply sufficient, and that it will distend the urethra for three and a half or four inches. A half-drachm syringe often suffices. It is scarcely necessary to say that these small instruments are much more easily managed by a patient than larger and longer ones. But most patients, unless specially taught to use the syringe, never introduce any injection at all. Unless the orifice of the urethra is carefully closed at the time, the fluid simply leaves the end of the syringe and flows out by the external meatus ; and in every case after the injection has been made, the moment the orifice is unclosed the fluid is rapidly expelled by the contractile force of the urethra, and no appreciable quantity remains within. So much, then, for any fear of its running down to the neck of the bladder. Of course, if an injection is too strong—in my opinion a very frequent occurrence,—the anterior part of the urethra is inflamed, and extension backwards may easily take place ; but that is no part of our subject now. But let me further say that, so far from your being able, even with the power of the syringe, to send an injection into the prostatic part of the urethra, you cannot do so by any ordinary force, unless you can at the same time voluntarily relax the muscles which surround the membranous urethra, and so allow the fluid to pass—a thing perfectly possible with very little practice to accomplish. Thus it is that you cannot inject the bladder except by passing an instrument into its cavity. In fact, this valvular passage stoutly resists all intrusion from without, and admits no fluid except in obedience to pressure which it is unable to resist.—*Lancet*, Nov. 27, 1875, p. 759.

53.—ON THE DIAGNOSIS OF DISEASES OF THE URINARY ORGANS.

By Sir HENRY THOMPSON, Bart., Surgeon Extraordinary to H.M. the King of the Belgians, &c.

I advise you always to pursue a uniform method. Order and uniformity are essential elements in directing the necessary investigation; and after much thought and experiment, for my own private guidance as well as for yours, I have adopted the following system. Relative to the class of diseases we are studying, there are three methods of obtaining the facts required:—Firstly, by questions of the patient; secondly, by physical examination of the body; thirdly, by examination, chemical and otherwise, of the secretions.

First, by questions. There are four chief questions which I always employ, and always in the same order. They ought, with the minor inquiries which branch out of them, to determine six out of seven cases which come to you. They relate to four signs and symptoms more or less met with in patients affected with complaint in any part of the urinary organs. Frequent micturition; painful micturition; deviation in the character of the urine itself from the healthy standard; the addition of blood to the urine.

The first question, then, which I invariably ask to any patient so affected is, "Have you any, and, if any, what frequency in passing water?" Then, as a branch of that question, springing out of it, I ask whether the frequency is more by day or by night, or influenced by movements, or by any particular circumstance? How the question applies I will tell you afterwards.

Then, secondly, I ask whether there is pain in passing urine, and whether before, during, or after micturition? Inquire also if pain is felt at other times, and if produced or aggravated by quick movements of the body. The locality of the pain is also to be precisely ascertained.

Then I ask, as a third question, "Is the character of the urine altered in appearance, or is there anything unusual in the stream itself? Is the urine turbid or clear?" Possibly the patient will tell you that it is turbid, but you find, on questioning further, that it was passed perfectly clear, and only became thick after cooling or standing. Also, as arising out of this, you may often ask, "Does it vary much in quantity?" noting of course the specific gravity. The healthy standard, both as regards quantity and density, however, must be allowed very extensible limits, and both, I need not tell you, are very important elements in regard of renal disease. Then, as regards the stream itself, it may be small, forked, or twisted, or it may stop suddenly when flowing.

The fourth and last question is, whether blood has been passed in any way with the urine; whether the mixture is florid or brown, light or dark; whether the blood and urine are intimately mixed, or whether the blood is chiefly passed at the end or at the beginning of making water; or, lastly, whether it issues independently of micturition altogether.—*Lancet*, Dec. 4, 1875, p. 793.

54.—ON PHYSICAL EXAMINATION OF THE URETHRA IN CASES OF STRICTURE.

By Sir HENRY THOMPSON, Bart.

[At the beginning of the present century, and the tendency still exists to a certain extent, instruments were employed too readily, and instruments also which are liable to injure the urethra.]

There are few structures more delicate than the male urethra. Formed of an internal membrane of great tenuity, surrounded by elastic and muscular fibres, interpenetrated everywhere with ducts, fine blood-vessels, and nerves; the whole surrounded in front of the prostate with one of the most elaborate of vascular structures, known as erectile tissue; add to this the sensitiveness to pain of the internal membrane, equalling the conjunctiva in that quality, as any one may learn for himself by passing the softest instrument he can find. Lastly, and more important still, it possesses another form of sensibility, of a much graver kind, which even the last-named membrane does not possess: I mean a strong tendency to arouse, in the entire nervous system, a state of excitement, evidenced by the striking phenomena of rigors and subsequent fever and prostration, when slight mechanical injury has been done to any portion of it. Now let this picture of the urethra as it is, never be absent from your mind's eye when you put an instrument into it, and both you and your patient will profit. When, therefore, a young man consults you for certain troubles relative to which you desire to learn whether urethral obstruction be a cause or not, do not be tempted for an instant to adopt so unnecessary a course (to say the least) as the introduction of very large instruments, or instruments with huge bulbs at the end of them. But simply take a flexible English gum-elastic bougie, well curved towards the point, with a blunt end (since a tapering point, of course, will not mark distinctly the site of stricture), not larger, as a rule, than No. 10 or 11 of our scale, and pass it very gently and slowly into the bladder. If it goes easily—above all, if it is withdrawn without being held, and slides out with perfect facility, take my word for it he has no

stricture, and, *quoad* obstruction, wants no use of instruments whatever.

I dare say I shall be told by some one, that there may be a urethra through which No. 11 can be made to pass, yet which is, nevertheless, the subject of some degree of stricture which it is necessary to treat. Well, I do not deny the existence of that exceptional case. But such an example is very rare, and, further, no man who has the slightest delicacy of tactile sense in his fingers can fail to observe the difference between a No. 11 bougie sliding out with facility, and one which is grasped somewhat as it is withdrawn. This difference is always manifest in the two cases, and is, indeed, very notable. But supposing that the No. 11 stops at any point, and you have assured yourself that it is not one of the natural obstructions to the passage of an instrument through the urethra, of which I shall have to speak hereafter, you will try a smaller instrument of the same kind until you have arrived at one which does pass through and inwards into the bladder. But you may also test your observation by passing a soft tapering French bougie—11 or 12 English size,—and if it is a *natural* obstacle which at first checked your blunt-pointed bougie, the former instrument will probably slip through without trouble.

Now, with reference to these bulbous-pointed flexible explorers, nothing is more easy, especially with an unpractised hand, than to deceive oneself with respect to the existence of stricture by means of them. If not drawn out accurately in the axis of the passage, you may feel a check, and readily find what you believe to be a stricture in the most healthy urethra. And that is precisely what I am so anxious to guard you against; for, in the hands of designing persons, these instruments may be turned to most unworthy purpose. For men outside the ranks of our profession, or holding a questionable position on its outskirts, these instruments are the very thing. That is an additional reason why we should be careful not to encourage their unnecessary use. I have used small bulbous instruments in metal for the diagnosis of narrow and confirmed stricture, on which it is desirable to operate, for twenty years, but for slight stricture never; and I have protested against their use on the two grounds already named; first, that for such the instrument inflicts needless pain; secondly, that it is liable to deceive a young operator not too adroit or familiar with its use.

But now, in the second case—not that of the young man, but of the patient who has a confirmed stricture,—the diagnosis has probably to be conducted with more attention to detail. I proceed as follows, making what may be called a “survey” of the passage. I commence by passing the soft, blunt-ended

English gum-elastic bougie as far as it will go; and when this stops, as it may do within an inch or less of the orifice, I make a note of the distance, and next find what will pass with tolerable ease through the obstruction met with. Very likely a No. 4 or 5 (similar instrument to the preceding) will do so, and it may be then carried further to seek another check, which is not unfrequently met with at about five inches from the orifice. Through this, after a trial or two, a very small gum catheter—say No. 1 or 2—may probably be passed into the bladder, drawing off some urine, and so assuring you of its position. I know then the patient has, at all events, a narrowing near the orifice and another at the distance named. I may verify this at once with a bulbous instrument if I choose; but as long as the anterior narrowing exists, it is as well to postpone more minute research until it is divided, as considerable narrowing at that part always requires to be. Such a one, indeed, is never advantageously treated by dilatation. This being done, the No. 11 blunt-ended bougie is passed, and we learn whether any contraction exists in the interval existing between it and the spot originally marked at five inches. If there is one, the stopping of the bougie will indicate its situation. But if the bougie passes easily to the point just named, the canal is sufficiently open, and we have arrived at what, in ninety-nine cases out of a hundred, is the only remaining stricture; for it is rare indeed, where a narrow stricture exists at that distance from the meatus, that any other will be found beyond. This has next to be dealt with, and, as we are speaking now of old and confirmed cases, there is little doubt that the best treatment will be internal urethrotomy. How to accomplish that most perfectly will be discussed in my lecture on that subject.

A word or two about the bulbous exploring instrument. It should, in my opinion, be of polished metal mounted on a slender metal rod or shaft. No other material slides so easily and smoothly through the urethra; and the necessity for it is, after all, not great, so that to employ one which produces much irritation, passes roughly, or distends unnecessarily, is to pay too high a price for the small amount of information it may convey. I am free to say that, although I used the metal bulbous instruments in the earlier part of my career, I can now, with more experience, attain all that is necessary without them. It is said that they enable you to ascertain the length of a stricture. In reference to this, let me tell you, first, that considerable narrowing almost never affects the urethra for any great distance. The passage is often partially implicated for half an inch behind and half an inch before the maximum point of narrowing—an important practical point in relation to operation; but the very narrow spot, which is what the exploring bulb indicates, is

almost invariably short, within a quarter of an inch in extent. Secondly, you must take into consideration the fact that the mobility of the mucous lining of the urethra will deceive you if you are not aware of it. A very little pressure in front, on entering the stricture, or from behind, in withdrawing the exploring bulb, if it comes rather tightly through, changes the situation of the contracted portion considerably in relation to its distance from the external meatus.

I repeat, then, if you have to deal with a recent stricture, such a one as you naturally intend to treat by dilatation, the introduction of a large bulbous explorer is wholly useless, only produces unnecessary pain, and tends to excite inflammation.

But in view of any internal incisions for a confirmed or obstinate case, the important thing is simply to know where the narrowed points of the urethra are, and then it is not difficult to apply your cutting instrument properly to them.—*Lancet*, Dec. 11, 1875, p. 827.

55.—ON THE BEST MANNER OF EMPLOYING INSTRUMENTS IN CASES OF STRICTURE WHERE MUCH DIFFICULTY EXISTS.

By Sir HENRY THOMPSON, Bart.

I have now to consider the question, How are you to deal with a case of stricture of the urethra in which real difficulty exists in passing an instrument into the bladder? Let a case be supposed in which attempts have been made, perhaps by several hands, and hitherto without success.

First, let me point out in what the chief mechanical difficulties in such a case might consist. There are four:—

1. Extreme narrowness of the stricture.
2. The stricture may be tortuous.
3. It may be complicated with false passages.
4. The urethra behind the stricture may be irregularly dilated and reticulated.

Difficulties of another kind, not mechanical, are frequently met with; these are two in number, and are to be considered hereafter—viz., (1) the stricture may be very resilient, and liable to become narrower, and to produce absolute retention on any instrumental contact; and (2) any use of the instrument may produce in the patient an attack of rigors.

1. Let us consider first the mechanical causes of difficulty, of which the first is extreme narrowness.

In regard to this, the first thing you must do is to see the patient make water. The failures to pass the instrument may not necessarily have arisen from narrowing of the urethra or from tortuosity; there may be a false passage in the canal.

It may, indeed, be that there is no stricture at all. No greater mistakes are made than those which occur with patients who have little or no stricture, either from the surgeon not knowing well how to pass the instrument, or from there being a false passage into which it enters, so that it does not reach the bladder at all. You are first, then, to see the stream of water, perhaps on more than one occasion, so as to estimate it fairly, and you will judge by that what size of instrument is to be used. And always let the instrument correspond with the size of the stream which you see; but it should correspond only in this way—the instrument should be a little smaller. You will sometimes see a patient who scarcely passes a stream at all; the urine issues only in a succession of drops. This being his constant, and not a temporary or accidental, condition, how very small must the instrument which is adapted in size to traverse fairly the stricture.

The first step to be taken with such a case, after having ascertained, as I assume to have been done, the situation of the stricture, is to introduce very gently the smallest English gum catheter without a stylet, and try to insinuate it through the obstruction. For this purpose I have endeavoured to obtain some exceedingly small instruments, much smaller than any hitherto made.* This tiny catheter, which I show you, containing a slender steel thread rather than a stylet, may be used either with or without it. The value of this little instrument, after its extreme tenuity, is its ability to transmit a drop of urine through its interior, and so assure you of its position when the bladder has been reached, an assurance, I need hardly tell you, of great importance. It is worth while making a careful and somewhat prolonged trial, and if you succeed, the instrument is to be tied in at once and the route secured. If I fail with this, I then try a silver catheter of the smallest size.

But there is one very important matter in relation to the silver instrument—namely, that it is an extremely dangerous one in inexperienced or in incautious hands. Nothing is easier than to implicate its slender point in a lacuna, and if the slightest force be used, the point leaves the route and finds a track amongst the loose tissues outside the urethral mucous membrane, and thus a false passage is commenced. If you cannot train your hand to employ it with extreme lightness and delicacy, by no means use it. It should be held and moved with so much tenderness, that if the point meets with any resistance in its course, the fingers rather should slip down the shaft than communicate pressure to the instrument. Any kind

* Messrs. Weiss and Son have taken great pains to accomplish this for me, and have produced some slender delicate instruments much smaller than No. 1, which have been of great service.

of force in its use is not to be thought of—its only value is in its capacity for being insinuated between closely applied surfaces, and any sense of opposition to its point is a clear intimation that its route may lay right or left, but not straight on. It is not fit for the hand of a tyro, but when he is habituated to the use of somewhat larger instruments, the ability to use this will follow. A careful trial, however, with the very small gum catheter, first without and then with the stylet, will in many cases make this unnecessary; but these failing, I know no instrument so valuable as a very fine silver one with a little increase of size in the shaft, to render it steady. The final three inches should not exceed half the size of No. 1 of the English scale; the shaft should gradually rise to fully No. 2.

2. A stricture may be more or less tortuous; in other words, it does not necessarily follow the exact direction of the urethra.

It may be a little on this side of the axis or on that, not necessarily in the middle. You may see this sometimes in the dead body; and you may infer it from experience on the living. When you have to deal with a very narrow or with a tortuous stricture, there are two quite distinct systems of using an instrument, either of which you may employ. What I have to say, then, applies to the manipulation necessary in all cases of difficulty under the present and preceding heads; and I have reserved it therefore until now. First, the little instrument employed, whatever its nature, may be applied by what may be called “groping”—that is, by carrying the point in any and every direction, with the utmost care, of course, until by chance it has entered into the stricture, as you may feel by the sensation of the point being “held” or “grasped.” For this purpose very slender solid instruments are made of gum-elastic, of catgut covered or not with gum, or of whalebone, since these can be produced of rather smaller size than the hollow instruments which I have just described. In order to add to the chance, as is supposed, of finding the orifice, there are some of the first-named material, the ends of which are formed somewhat into the shape of a corkscrew, or are otherwise made to deviate from a straight line. All these are of French design and workmanship, but none have the advantage of being hollow, to demonstrate their presence in the bladder when they arrive there.

Now, I advise you not to rely on mere groping to find the orifice; it is exceedingly dull mechanical work to be constantly groping for a considerable time on the chance of success.

There is a method of search, however, conducted on a fixed principle, which I much prefer and always employ. I advise

you to adopt that, or any other you can devise, provided that it be a systematic one, and also an exhaustive manner of making the necessary exploration and researches for the orifice of the stricture. Some of you have often seen me apply it to patients in the wards upstairs. According to this manner you proceed systematically and examine each side of the urethra *seriatim*. We will suppose the urethra to require examination on four sides—a roof, a floor, a right side, and a left side. You are to take your slender silver catheter and slide its point delicately along each side successively from the orifice to the place of obstruction. If the orifice of the stricture is not *exactly* in the middle line, and we may fairly believe that it rarely is so, then there is one side of the passage which will more easily lead into the narrow way than any other. If I go down on this side I shall probably not get into the stricture; but if I slide down gradually on the other I shall probably get the instrument in, because you see there is less obstruction on that side. Begin, then, by the roof. The roof is the firmest part, the least likely to furnish obstruction in the way of ducts or lacunæ, and by following it you will be most likely to carry the point in. The floor, on the contrary, is the softest, loosest, and most spongy part, and will be most likely to yield to the instrument and give way. If your first effort does not succeed, take the right side; if that does not do, take the left; if that fails, try the floor. If you are very careful you may make the attempt in this manner for thirty or forty minutes without doing any damage; but if the patient suffers, or if he bleeds, or if you are losing patience, give up the task, or you may make a false passage, and increase the difficulty considerably.

3. The existence of a false passage leading out of the urethra is another serious obstacle to the introduction of instruments into the bladder.

The principle which must guide your action here is to be very careful to avoid the side on which the false passage is. A false passage commences usually on the floor, and no doubt for a reason already mentioned—viz., the fact of the structures below being looser and more delicate than those above. When you examine a patient with a false passage, you may find the instrument passing to its very end, notwithstanding which no urine flows. Hence the false notion of stricture at the neck of the bladder. When the instrument has thus passed, put your finger into the rectum, and you will know instantly whether there is a false passage; for, if so, there are only the coats of the bowel, which are very thin, between your finger and the instrument, so that you feel it very distinctly; and very commonly not in the middle line, but rather to the right or to the

left. But if the instrument is in the right passage, the whole thickness of the prostate, not always very considerable, is felt between it and your finger. It is almost always in the bulbous portion that the instrument leaves the urethra and gets under the prostate. What you are to do, then, is to withdraw the catheter two inches or so, pass it on again, keeping as close along the upper part of the urethra as you can, ascertaining by means of the finger in the bowel that the instrument is not passing into the old route. But when a false passage exists, it sometimes constitutes the chief obstacle to the introduction of an instrument, for, as I have before hinted, the stricture itself may be only inconsiderable. The size of the stream will help to determine this point; and if, so far from being a mere thread it has a volume equal to a No. 6 or 7 catheter, use a No. 5 silver one, so that you can guide it, and try each side of the urethra successively until you discover which it is that enables you to clear the orifice of the false passage. Remember the result, and you have a key to the difficulty which will enable you to overcome it with ease on the next occasion.

4. Difficulty presented by dilatation and reticulation of the urethra behind the stricture.

Suppose next, that, following these hints, you have carried the instrument through the stricture; there is a sort of grasp felt on attempting to move the instrument which is quite unmistakeable. But that very "grasp" which you are so satisfied to feel, makes it less easy to manipulate the point of the catheter after it is through the stricture, and you may have a source of danger to encounter in the urethra beyond. Thus the mucous membrane, being sometimes reticulated from the presence of dilated lacunæ, it is easy to engage the point of the instrument in one of these, and make a false passage. Besides this, as the result of long continued fluid pressure and straining, the urethra behind the stricture is occasionally much dilated, and the whole surface is so irregular that it requires care and an acquaintance with this condition to traverse it safely. Here especially, with a small instrument already firmly grasped, you may require all the caution you can command in completing its safe course into the bladder.—*Lancet*, Dec. 18, 1875, p. 867.

56.—ON INTERNAL URETHROTOMY, AND ON THE INSTRUMENTS EMPLOYED FOR PERFORMING IT.

By Sir HENRY THOMPSON, Bart.

There are various ways in which the operation of Internal Urethrotomy for Stricture may be done, and there are numerous instruments for the purpose. There are two different principles

of action in making the necessary incision, and the instruments may be grouped in two classes by this means.

I. The stricture may be cut from "before backwards" on a guide previously passed.

II. Or a blade may be introduced through and beyond the stricture, which is then cut from "behind forwards," in which case a guide is unnecessary.

Before considering these, I may state, that the situation of a stricture is an important element in relation to the applicability of internal urethrotomy of any kind, and also as to its necessity as a mode of treatment. Thus:—

1. A stricture at or near the external meatus is always extremely undilatable, while at the same time it may be divided with the greatest ease, precision, and safety.

2. A stricture existing in any part of the anterior three or four inches of the urethra partakes, but in a somewhat less degree, of the intractable quality named, and may be cut with almost as much facility and safety as those in the previous class. As a rule, it may be said, the nearer a stricture is situated to the orifice of the urethra, the more necessary it is to cut, and the safer it is to do so.

3. Strictures in the bulbous portion of the urethra, which may be considered as generally ranging between four inches and five inches and a half from the external meatus, are less liable to be unmanageable by dilatation than those of the preceding classes. But when necessary to cut them, it is easy and quite safe to do so as a rule, slightly less so than those in the preceding class; the difference being due, no doubt, to the larger amount of erectile tissue there as compared with the anterior part of the urethra. Bleeding, therefore, may be more considerable, and other risks, such as they are, more liable to be encountered.

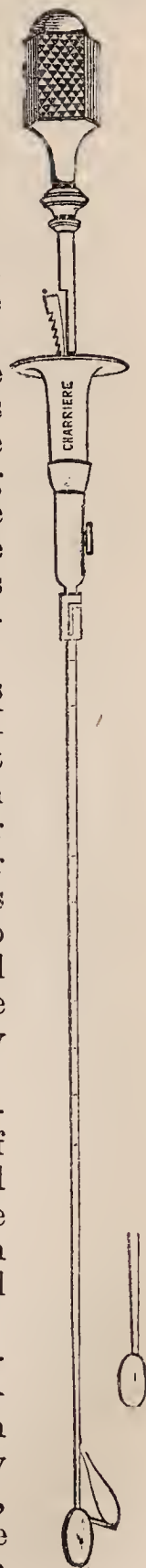
Now, suppose for an instant that it were possible for you to have the stricture before you open to your eyes and hands; you certainly would take a little scalpel and employ it for each case according to its needs, intelligently dividing the obstructing fibres as much as is necessary, and no more. But with a very little practice it is perfectly easy to do this without seeing the stricture—that is, to apply a little scalpel, dividing where you wish and not elsewhere, just as you divide the constricting fibres at the ring of a hernial sac. And I need scarcely say that this intelligent action of yours will produce something better than the work of a machine which any ignorant person can employ just as well as you. The difference in the two cases resembles that which exists between the music of an organ produced by a handle, and the music which results from the facile hand of a skilled performer.

It is a question of no mean importance which arises from the comparison of incision made by machinery and incision made by the intelligent hand. That is the real question in urethrotomy. And the instrument, which I always use, and have operated with so many times, is the type of the method which affords the opportunity of making the incision according to the operator's will and judgment. It is nothing more than a little knife with a long handle, and is used precisely as we use a scalpel anywhere else. Just as we should use a small knife in tenotomy without the sense of vision, where it is not necessary, but guided by the sense of touch, so do I advise you to cut in urethrotomy. I carry the parallel one step further, and remind you, that just as it is essential in tenotomy to divide completely the tendon, and also any little minor band of constricting tissue which opposes the perfectly free play of the joint, so is it essential to make the same complete division also in urethrotomy. The ultimate success in either operation depends on attention to these particulars.

The instrument to which I have been referring is the urethrotome of Civiale, made much smaller, as I prefer it to be, than is customary in Paris. The shaft of mine is about the size of No. 3 English scale, with an oblong bulb at the end not larger than No. 5 or No. 6. Within this is a little blade, which, by a clever contrivance, the operator can make to issue at will, as much or as little as he desires, and which he can also sheath instantly at his pleasure. The bulbous end gives him the power of exploring the urethra at the time of the operation, and of determining precisely where and what he ought to divide. (*See Fig.*)

The bulb is to be introduced, say fully half or three-quarters of an inch beyond the maximum point of narrowing felt, the blade is then to be projected there, and drawn steadily through the whole of the constricted portion outwards towards the meatus, in which direction the incision is sure to be certain and complete.

Now I at once anticipate an objection which I expect you all to raise. I expect you instantly to exclaim, "Why, if an instrument so large as No. 6 can be passed through the stricture, should there be any occasion to cut at all? Surely it is for small strictures, through which only the finest instruments can be passed, that an operation is necessary, and for such



this instrument of Civiale is obviously unfitted by its size?" Such criticism is quite natural at the first glance, and from those who have not learned by considerable experience certain important facts about confirmed stricture.

I reply, first, that the *narrowness* of a stricture alone, however extreme, does by no means render a cutting operation *always* necessary. Some of the narrowest strictures I have ever seen have been very successfully and rapidly treated by simple dilatation.

Secondly, the quality which makes incision of a stricture necessary is, as I have previously shown, "resiliency"—that property which leads it to contract again rapidly after any dilatation. This quality affects strictures which will admit No. 5, No. 6, or No. 7 quite as frequently as narrower contractions.

Thirdly, supposing it is necessary to cut a stricture which is narrow and only admits No. $\frac{1}{2}$ or No 1, there is *never any difficulty in dilating it temporarily* up to No. 6 for the purpose of admitting the instrument I recommend.

All you have to do is to keep the patient in his room from three to five days with a small soft gum catheter tied in, which produces scarcely any inconvenience; and thus by "continuous dilatation" you can always bring the most resilient stricture to the required size for the operation. Having arrived at No. 6, you are enabled to perform the operation in the best possible circumstances. And the few quiet days spent in preparation are advantageous rather than otherwise; the result furthermore being that you can now make an incision in the manner I have described, directed by your intelligence and adapted to the particular case.

For safety to the patient and excellence in result, both in regard of his present and future condition, I greatly prefer the operation I have described to any other method I have ever seen.—*Lancet*, Jan. 1, 1876, p. 1.

57.—ON SOME IMPORTANT POINTS IN CONNECTION WITH LITHOTRITY.

By Sir HENRY THOMPSON, Bart.

[Modifications of practice have gradually taken place as the result of experience, which have removed most of the old objections to the operation of lithotrity. The success has greatly increased, and the after results much improved.]

1. The first principle I wish to inculcate in connection with the management of patients during lithotrity is, that it is desirable to treat any cystitis that may occur during the course of the sittings, especially if it is severe, by freely crushing the

stone without delay. I have for some years insisted on the importance of this practice in my teaching. I believe that in this country its value has long been to some extent recognised; but the more I see of the operation, the stronger is my conviction of the great importance of this practice, and the more ready I am to adopt it. Abroad, cystitis is more generally treated by baths, diluents, low diet, and medicine; and the next sitting is postponed until the patient may be, as it is said, in better condition for the instrument. The surgeon fears the effect of introducing a lithotrite into the inflamed bladder, lest the disturbance should increase the inflammation—a result which it appears at first sight not unreasonable to expect. Now the precisely contrary effect is that which occurs. For example, I crush a uric-acid stone once; or it may be twice, with two or three days' interval between. In thirty-six or forty-eight hours after the sitting the pain and frequency of passing water become considerable, the urine is loaded with muco-purulent matter, and it assumes a full-red tint. Some slight febrile condition may or may not be apparent in the patient. Under these circumstances I place him under the influence of ether, crush freely all the large and sharp pieces, and wash out the débris with Mr. Clover's aspirator. Six hours after the urine is much clearer, the muco-pus has almost disappeared, and the patient's general condition is greatly improved. This will happen certainly in nine out of ten of the cases described. I have had the opportunity of demonstrating this to many of my foreign brethren, who are invariably struck with the result. The fact is that the cystitis in these cases is purely a traumatic one, that it is due to the injurious effects of the sharp, hard portions of stone which lie in continuous and close contact with the mucous lining of the bladder, the result of which is that incipient ulcerative action takes place. Crush and remove these, and that instant repair commences, inflammation is rapidly diminished, and the patient is no longer in danger, but is greatly relieved. Even simply changing the situation of the large pieces in the bladder is followed for a few hours by manifest improvement, showing in what manner their presence is injurious. If some large pieces still remain after the fresh crushing, the former symptoms of cystitis reappear in two or three days, and may be again relieved by another sitting, this time probably altogether or nearly so, as a succeeding crushing will, no doubt, dispose of the principal part of the stone, and leave at all events no considerable fragments. This practice has, moreover, an important relation to the after-condition of the patient, as we shall see by and by.

On the other hand, let us suppose a case of lithotrity in

which all goes well with the patient, and no fever, no cystitis, &c., happen; still the interval between the sittings should be short, not more than two or three days. You take advantage of the tranquil state of affairs to proceed steadily and carefully, not tempting fortune, but without a day's unnecessary delay. But if cystitis appears you are, as already advised, still less to lose time; and so in either event the operation of lithotrity is soon completed. I rarely exceed now fourteen or sixteen days as the full term which includes all the sittings. Often eight, ten, or twelve suffice.

This practice is no doubt facilitated by the recent improvements in anæsthesia. I have never been, as you know, very well disposed towards the employment of chloroform for any purpose when it could be dispensed with, but I have of late, mainly owing to the improved methods of producing anæsthesia, availed myself of it much more than formerly. I refer to the system which Mr. Clover always pursues, of administering first nitrous oxide for about thirty seconds, and then following with ether vapour. The rapidity and greater safety of the process as compared with that by chloroform, together with the freedom from subsequent sickness usually attained (the latter an advantage of no slight value), have influenced my practice, and I now make anæsthesia the rule and not the exception.* Consequently, also, I avail myself of the opportunity to do a larger amount of crushing than I did when the patient was conscious—a course which, while it is permissible or advantageous in practised hands, is not to be necessarily recommended to the young operator. To him, now as ever, I repeat that success can only be obtained by the greatest caution and prudence. He must not be tempted to introduce the lithotrite so frequently as he sees that I sometimes permit myself (especially in cases of cystitis requiring immediate relief) to do. I still advise you to be content with two, at the most three, introductions of the instrument at a sitting, and thus much only if you are able to make them with tolerable facility.

2. The second principle which is to guide your practice in lithotrity is the importance of detecting at all stages of the procedure any inability on the part of the patient to empty his bladder by his own efforts. Let me say at once that a very little negligence in relation to this matter is the chief cause, beyond all question, of the subsequent troubles which occasionally become evident at an early period after the stone has been removed, and which have constituted, in a certain propor-

* Mr. Clover has employed this method, more or less, for me now about two years and a quarter, and for the last year and a half has done so without exception, using no chloroform whatever. In reply to my inquiry, he writes me that he has during this period administered the gas and ether for me two hundred and thirty times for lithotrity.

tion of cases in elderly men, sources of discredit to the crushing operation. In the cases referred to, the calculus is often removed easily enough, little or no trouble occurs during the procedure; but it becomes evident afterwards that the frequency of passing water is not remedied, and small pains and discomforts continue. The patient leaves his surgeon, and reappears in a few months or even weeks, with clouded, perhaps alkaline, urine, depositing triple phosphates, and with other symptoms more marked than when he left. My friend, Mr. Cadge, who has been reporting of late his valuable experience in relation to this matter, regards these phenomena as not unfrequently due to the escape of a small fragment into a sacculus in the bladder, where it becomes the source of irritation, phosphatic deposit, and occasions the formation of a fresh vesical calculus, with ceaseless discomfort and repetitions of crushing for the patient. I agree with him that this sometimes happens, having had, like him, ocular and other demonstration of the fact; but I have no hesitation in saying that in the large majority of cases this is not the cause, and that, happily, it is due to another cause, one that is in a very great degree preventable. These after-troubles will appear in patients who have had very little uneasiness antecedent to the operation. Their stones have been small, and their bladders have been in excellent condition; no ground, therefore, can exist for supposing such a patient to be the subject of sacculus in the bladder. Every presumption, indeed, is opposed to that view, because sacculi, as a rule, do not form without the existence of an obstruction to the outflow of urine, which has been in operation for some long period of time.

Now the particular fact which I am going to state is one I was scarcely aware of five years ago. I have long been aware of the necessity of watching carefully for inability to empty the bladder during lithotrity, have long also spoken of the insidious manner in which that inability commences, and pointed out that it must be dealt with by the habitual use of the catheter. But I have only learned during the last few years how extremely small a quantity of urine habitually left behind in an elderly patient's bladder after each act of micturition, provided that he is undergoing, or has just undergone, lithotrity, suffices to lead to phosphatic deposit and to chronic cystitis. You will scarcely believe me that one drachm, or one drachm and a half, only of this residual urine is enough in most such cases to produce the condition so feared and detested by every lithotritist; and I now add, that if the condition is detected early, and this small quantity is promptly and frequently removed by the patient himself, which he can easily do with a soft *coudée* catheter, almost certainly the dreaded symptoms will not appear.

I have been surprised at the result; not less surprised than I have been at the fact that the patient, who towards the close of lithotrity, or afterwards, is making urine every hour or so, and who, on passing his catheter, finds only one or two drachms behind; often obtains at once an interval of three hours or more. Such a one should pass his instrument at least three times a day until he regains the power to empty the bladder completely by his own efforts. I confess that formerly so small a quantity of urine did not seem to me worth the trouble of removing; as I assumed, on theoretical grounds of course, that it could not be of any importance. I now know that the practice of removing it constitutes for many cases the difference between a permanent success and an ineffectual result and painful future.

Now then, how does the plan of crushing for cystitis, referred to under the preceding head, affect, as I said it would, also this important question which we are now discussing? Thus: it is precisely in those cases where the treatment has been prolonged, or where cystitis, either acute or chronic, has been allowed to go on unchecked, that the inability to empty the bladder is most likely to occur. Again, this inability, once commenced, very rapidly becomes the established order of things in elderly people, unless it is checked at the outset. Once let the bladder be accustomed to the smallest degree of accumulation of urine, and the power to empty itself entirely is, after a certain age, often permanently lost. The best chance of preventing this, and the phosphatic deposit which results, is, in the first instance, to avoid or cut short cystitis during the treatment by lithotrity in the manner described; and, secondly, to teach the patient to empty his bladder himself towards the close of the operative proceedings or immediately after, if the smallest failure to empty it is detected. Once for all, let me say, I cannot exaggerate the importance of these recommendations.

Now, when this tendency to produce phosphatic deposit has been unfortunately established, we are often able to benefit the patient by teaching him to wash out the bladder, by means of an ordinary catheter and bottle, with weak acetate of lead, nitric acid, &c. But of late I have adopted another method, with manifest advantage. I advise the patient (who may or may not be habitually using the catheter to empty his bladder) to use every second or third day the following apparatus:—

(1) A black flexible catheter, No. 11 or No. 12 in size, made thin, and with polished interior, so as to facilitate the passage of débris through it (a great improvement on the ordinary French flexible catheters, which are thick and have often rough interiors), and having a large oval eye on the upper surface of its extremity, which is slightly turned upwards (*coudée*).

(2) An eight-ounce india-rubber bottle, with a brass nozzle which fits *over* the outer end of the catheter, and not into it. The manifest result of this mode of attachment is, that a powerful uninterrupted current can enter and issue from the bladder; indeed it is scarcely possible that *débris* should remain in the organ under the influence of the action of this apparatus, as any one can perceive on using it. It may also be used as an aspirator, with a backward and forward current, if desired. For the patient's own use it is even more easy than the ordinary four-ounce bottle, which I have long been in the habit of desiring such to apply, since the size and freedom of the channel—not narrowed at the point of contact between bottle and catheter—permit the fluid to be propelled with very slight pressure.

There is still one admirable remedy for that low chronic cystitis which is associated with the production of phosphatic calculus in the bladder—viz., the injection, every day or every other day, for a short time only, of a very weak solution of nitrate of silver. About half a grain to four or six ounces of distilled water is amply strong enough; and one or two injections sometimes suffice, in these circumstances, to produce a very notable diminution of the muco-purulent secretion. — *Lancet*, Jan. 8, 1876, p. 39.

58. — ON THE RETENTION OF BOUGIES INSTEAD OF CATHETERS FOR THE CONTINUOUS DILATATION OF STRICTURES.

By FURNEAUX JORDAN, Esq., Surgeon to the Queen's Hospital, Birmingham.

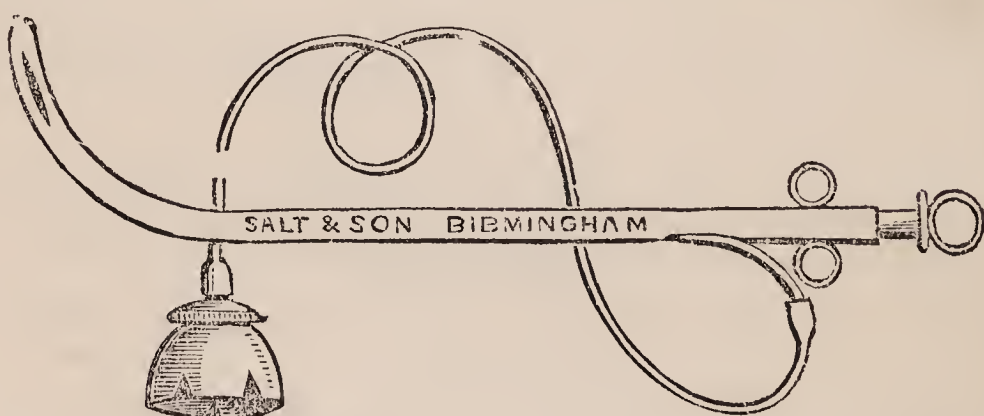
Among the various methods of treating stricture of the urethra, that of continuous dilatation is justly held in much esteem. In a few days, and with no risk, a very tight stricture may be dilated so as to permit a large instrument to be passed; hence the treatment may be said to combine the safety of gradual dilatation with the rapidity of incision or rupture. It is not suited to every case. It will not, for example, take the place of internal division in strictures anterior to the scrotum, nor of external division in persistent fistulæ. In a large number of tight—perhaps very tight—old and neglected strictures, with no great irritability or resiliency or attendant induration, the surgeon is glad to get in a fine instrument and leave it in.

Although it may require much patience and skill to introduce the finest bougie, the general custom is to withdraw it and attempt the introduction of a catheter. The effort occasionally fails, and much valuable time is lost. Now, in such cases I have for several years *kept in the fine or filiform bougie, and*

directed that the urine should be passed from time to time by its side or around it. In every instance in a large number of cases this has been easily done, micturition, as a rule, actually being easier and in a larger stream than when the bougie is out. In twenty-four hours a goodly-sized bougie (not catheter) can be introduced. On the third day I usually put in a No. 12 or even No. 13 bougie or catheter. It is of course in the earliest stages that the superiority of the bougie over the catheter is seen.

The advantages of the treatment I have just described are these:—A more rapid and complete dilatation, due to the hydrostatic pressure of the urine along the exterior of the bougie; a bougie is more easily introduced than a catheter; when the finest bougie is once in, it need not be taken out—no slight boon; the ordinary acts of micturition are preserved; every kind of apparatus for keeping the bed dry, or for any other purpose, may be dispensed with.—*Lancet*, Jan. 29, 1876, p. 169.

59.—IMPROVED CATHETER FOR BED-RIDDEN PATIENTS.



Messrs. SALT & SON, Surgical Instrument Manufacturers, Birmingham, write to us:—"In a paragraph which appeared in the *British Medical Journal*, January 1st, 1876, you mention a suggestion of Dr. George for rendering a catheter more convenient for use with bed-ridden patients, by the attachment of a piece of india-rubber tube connecting the catheter with the *pôt de chambre*. The engraving above will show the plan we recently devised for accomplishing this purpose. To an ordinary catheter we attach a curved tube, on the end of which the india-rubber tube is slipped, having at its other end a small weight to prevent the tube from leaving the vessel. The distal end is made to act as a syphon; the flow only takes place when the sliding tube is drawn out about half an inch. By this arrangement, the bed-clothes and the surgeon's hands are protected from the fluid."—*Brit. Med. Journal*, Jan. 8, 1876.

60.—ON PUNCTURE OF THE BLADDER *PER RECTUM*.

By RICHARD DAVY, Esq., Surgeon and Lecturer on Anatomy at the Westminster Hospital.

[There is scarcely any operation so devoid of danger as puncture *per rectum*. The instruments usually used are a solid trocar and cylindrical canula, the latter being retained in the bladder by tapes.]

In 1870, being impressed with the disadvantages of retaining for any length of time an unyielding tube *per rectum et vesicam*, I introduced the instrument (Fig. 1), in which the steel slotted

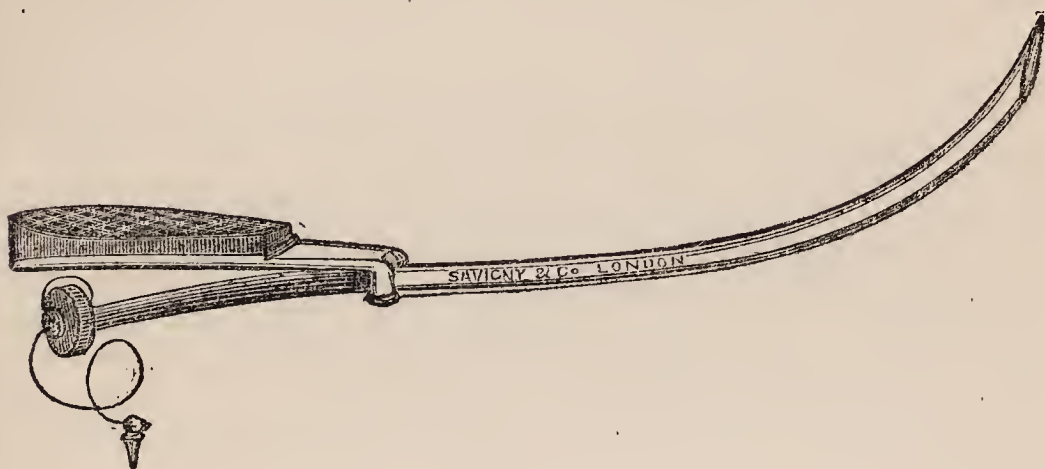


Fig. 1.

cylinder is outside, and acts as a perforator, carrying the elastic catheter as an inside passenger. Fig. 2 shows the catheter withdrawn, also the reduplication of the tip of the catheter, by means of which it is retained in the bladder. The patient is placed after the operation on a mattress having a circular hole corresponding to the buttocks; the end of the catheter falls through this hole, and conducts the urine into a receiving vessel on the floor below. Fæces also are thus passed without moving the patient.

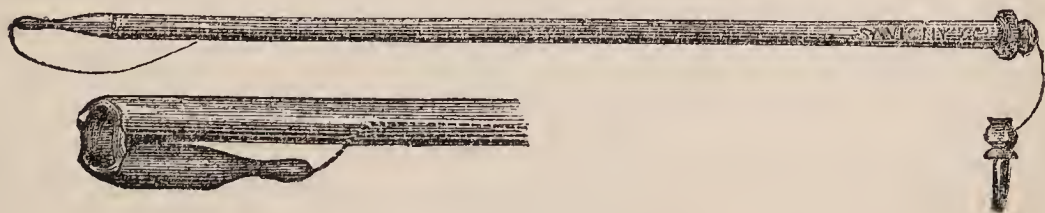


Fig. 2.

I have yet further applied this principle of giving the urethra rest to those cases of obstinate perineal fistulæ that are *not dependent* upon an obstructed urethra for their persistence. I am of opinion that a simple operation like puncture *per rectum* is more effectual and easy than periodic introductions of a

catheter ; the object being in both cases to avert the irritating urine from the damaged urethra and fistulæ, especially so because in some cases men have not the power of retaining their urine ; and it is impossible by the most skilful catheterisation to avoid some disturbance to the urethra, and some extravasation of urine over the exposed surface of it and the fistulæ.

I have been in the habit of comparing the *rationale* of puncture *per rectum* in fistulous cases to the method pursued by workmen when repairing the damaged banks of a running stream. Their first object is to completely divert the stream from the seat of their repairs, so that one day's wash shall not outweigh one day's work ; and this argument applies with double pathological force to the irritating urine as compared with the pure water. But a moment's reflection makes a surgeon see how inapplicable Cock's method would be for opening an empty bladder ; and, at the risk of the charge of superfluity, I venture to bring you acquainted with a new instrument, and a new method of operating on this much operated on human perinæum. The instrument consists of a staff, on which slides a silver cylinder, and a removable handle. (Fig. 3.) The silver tube runs from the bulge at the right end of the staff to the commencement of the curve on the left. The thin stem underneath the handle is for the attachment of a retentive catheter.

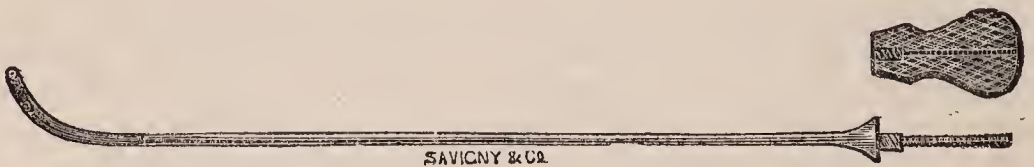


Fig. 3.

New Method of Operating.—First introduce the staff into the bladder ; turn the tip of the staff towards the base of the bladder and rectum ; feel for the point with the finger in the rectum, and carefully cut the recto-vesical tissues until you can pass the staff out at the anus. Next unscrew the handle and affix the stem of one of Napier's retentive catheters to the thin end of the staff ; soap well the india-rubber ; then, holding the silver tube firmly in one hand, draw the catheter (excepting its bell-shaped end) completely into the silver tube by withdrawing the staff at the anus. Lastly, draw back the silver tube and catheter through the urethra into the bladder ; push in the catheter, and free the silver tube by also withdrawing it through the rectum out at the anus. The campanulate end of the catheter unfolds itself in the bladder, and its stem loosely hangs at the anus.—*Brit. Med. Journal*, Dec. 4, 1875, p. 697.

61.—ON THE TREATMENT OF ACUTE ORCHITIS BY PUNCTURE OF THE TESTIS.

By HENRY SMITH, Esq., Surgeon to King's College Hospital.

[Since the year 1864, when Mr. Smith brought his plan of treating acute orchitis before the profession, he has performed the operation in more than one thousand cases. Hundreds of Mr. Smith's pupils at King's College Hospital have witnessed this treatment, and thus the results have been pretty satisfactorily ascertained. It consists in puncturing the body of the inflamed organ by a single incision extending in depth to half or three-quarters of an inch.]

It is only in cases of the most acute form of the disease that I recommend this treatment, and in by far the majority of these instances I have found that the relief given to the pain in the testicle itself is very marked, and almost instantaneous; in fact, so much so that patients who come to the hospital in severe agony are enabled to walk out of the building comparatively free from pain, and in many instances joking about the treatment to which they have been subjected. I have had patients return to me for similar treatment to the other testis, after undergoing the operation on the one at some previous time, and than this there can be no better proof of their estimation of the relief which is produced by the puncture.

When questioned by pupils as to the *modus operandi* of the remedy, I have answered that the sudden relief from acute pain is due to the circumstance that the unyielding tunica albuginea is freely incised, and that the tension is thus taken off the compressed and swollen testis; in some instances there is more or less serum effused into the cavity of the tunica vaginalis, and the withdrawal of this greatly assists in giving relief, but in many instances there is hardly any effusion of serum. In most cases undoubtedly the loss of blood produced is an important factor in the rapid subsidence of pain, but in some cases so little bleeding is produced that the relief cannot be explained in this way. Vidal de Cassis explained the sudden relief by the compression being taken off the testis through division of the tunica albuginea. In the old-fashioned treatment which I was taught to adopt, large quantities of leeches and nauseating doses of antimony were the chief ingredients; yet nothing like the speedy and effectual relief from puncture was produced, and every now and then a violent attack of erysipelas would be brought about by the leeches. As a general rule the puncture is productive of so much relief to pain that no other treatment is required, except to support the part, and occasionally to use compression by strapping—a means well calculated to diminish

the swelling, which is scarcely at all reduced by the puncture, and the persistence of which annoys the patient.

I have seen none of the bad results from puncture as have been pourtrayed by the fertile imaginations of alarmists, who were haunted in their dreams by visions of hernia testis, atrophy of the organ, and emasculation. I had once a case of a slight protrusion of the "tubuli seminiferi," well known to my pupils; but this occurred in a young strumous man who, on the day following the operation, received a severe blow on the scrotum.

I have not met with a case of atrophy of the testis, but I have seen two cases in private practice, in young healthy persons, where the testis was quite destroyed from the violent inflammation of the organ, and where the good old-fashioned plan of leeches, &c., was used; in all probability puncturing the testis in these cases would, by relieving the inflammation, have prevented the destruction of the organ.—*Lancet*, Jan. 8, 1876, p. 43.

62.—PUNCTURE OF THE TESTIS IN ACUTE ORCHITIS.

By T. W. NUNN, Esq., Surgeon to the Middlesex Hospital.

A case of acute orchitis occurred in my practice at the Middlesex Hospital, in which I had failed to relieve the patient of the intense pain he was suffering. The testicle was not greatly swollen, but it was very hard; and the scrotum, which was thin, was unusually ruddy. I punctured the testicle, at two or three points on its anterior and outer aspect, with a bistoury one-tenth of an inch wide, to the depth of about four-tenths of an inch. The pain almost immediately began to subside, and convalescence commenced. I punctured the testicle, acting on the same principle that I should perform iridectomy in acute glaucoma, and I assume it is on this principle that Mr. Smith bases his treatment. Those who have watched the progress of ophthalmic surgery during the past five-and-twenty years will remember the prejudice that existed against iridectomy—a prejudice that has yielded to extended experience, but a prejudice which doubtless beneficially acted in controlling the proceedings of the mere imitators of the operations of truly scientific surgeons; I therefore venture to hope that this question of puncture of the testis in acute orchitis will receive a calm and fair consideration. In my humble judgment it is a question that involves important surgical principles, and is not to be settled either by ridicule on the one hand, or by limited personal experience on the other.—*Lancet*, March 25, 1876, p. 479.

63.—ON THE TREATMENT OF INCIPIENT STRICTURE BY OTIS'S OPERATION.

By BERKELEY HILL, Esq., Professor of Clinical Surgery
in University College.

[What is known as "gleet" is produced by affections of very different nature and in different parts of the urethra, and requires very different kinds of treatment for their cure.]

Last July Dr. Fessenden N. Otis, a distinguished surgeon of New York, demonstrated in our operating theatre his mode of curing gleet and stricture of the urethra, in a lecture remarkable for its clearness and for the skill with which his manipulations were performed. In that exposition Dr. Otis enunciated views which vary considerably from, and indeed are opposite to, the doctrines usually taught in this country. I propose to-day to examine what we were told on that occasion with the light that some experience we have been able to gain in our own hospital has thrown upon it. In doing this I would not have you suppose that there was little to be learned from our American *confrère*; on the contrary, I am satisfied that much of what he told us is perfectly true, and a real contribution to our knowledge of urethral affections.

The chief points of Dr. Otis's demonstrations were:—

1. The human urethra varies much in its calibre in different persons. Hence an instrument that is a full size for one man may be either much too great or too small for another.

2. The urethra is really much wider than is generally taught.

3. The meatus urinarius is normally as wide as the rest of the canal.

4. Gleet is always due to stricture. It is "the signal that nature hangs out to call attention to the fact that stricture exists somewhere."

5. In the term stricture Dr. Otis includes those early indurations which have not sufficiently advanced to interfere with the passage of urine or to produce any symptom beyond a discharge. But he maintains them to be really bands of contractile tissue-fibres produced by inflammatory action.

6. Stricture is most frequent in the first inch from the meatus, and is less frequent as the distance from the entry increases.

7. Complete division of a stricture and maintenance of the incised part at its natural width until the incision is thoroughly healed prevents return of the contraction, and, moreover, *cause absorption of the indurated tissue from the affected part.*

I propose to examine these novelties one by one.

That the urethra should vary in calibre in different persons, considering that the penis also varies greatly, might well be presumed *à priori*; and Dr. Otis has most satisfactorily demon-

strated that it does so. But I trust you have not forgotten the description of the urethra given by Sir Henry Thompson in his lectures before Christmas. He there showed how the urethra is not a tube at all except while some body is passing along it, and defined it to be a *closed valvular chink*, capable of distension to a different amount in different parts of its length. It will be well to bear this in mind, and also that for our purpose to-day we are concerned only with the *spongy portion* of the urethra.

Books on anatomy tell us that the bulbous portion is somewhat wider than the rest, having a circumference of seven-tenths of an inch, and that the remaining part is one or two tenths less in circumference; further, that the meatus does not exceed one quarter of an inch in width. These measurements are doubtless taken from the dead body, and if we conclude that they represent the dimensions of the living urethra, we shall be in error. The practical importance of ascertaining what is the usual extent to which the "valvular chink" is dilated during micturition is this: diminution of capacity in the urethra means impediment to the flow of urine from the bladder. If the balance between the natural expulsive force of the bladder and the friction of the stream along the urethra is disturbed, the bladder is irritated, the kidneys are affected, and the beginning of the long chain of events, which terminate not infrequently in death, is made. To know if a man has stricture, we must first know what the natural distensile power of his urethra is, and to ascertain this, Dr. Otis discarded any arbitrary standard, which, you know, is usually set at about No. 12 of the English scale, but measures each urethra before he proceeds to operate upon it. For this purpose he has invented a most ingenious instrument which he calls the "urethrometer." It consists of a slender cannula marked in inches, at the end of which a set of steel springs can be expanded into a bulb by advancing a stem within the cannula. This movement is obtained by turning a screw at the handle, and the amount of expansion is recorded by an index on a dial-plate. When screwed close the instrument is not larger than a No. 6 English sound—that is, a circumference of less than half an inch. It can be expanded to a maximum circumference of an inch and three-quarters—two and a half times the seven-tenths of an inch which your anatomical guides tell you is the circumference of the widest part of the spongy portion. To measure the urethra the expanding sound, covered by a thin india-rubber sheath is introduced in its contracted form as far as the bulb, between five and six inches. It is then screwed up until the patient announces he has a sense of fulness, but not so tightly that the instrument cannot be moved without being grasped by the

passage. This gives the size of the canal at the bulbous part. The urethrometer is gently withdrawn, the expanding part being enlarged or diminished as tight places or slack ones are passed, and the several dimensions are noted by observing the index, and the distance of the expanding part from the meatus. Any diminution from the widest measurement Dr. Otis holds to be a stricture and abnormal. For the further examination of these contracted areas, Dr. Otis employs a series of bulbous sounds ranging from about No. 4 English catheter to one much greater than any size in our scale. But before describing them I must remind you that Dr. Otis, like nearly all who work at urethral affections, has discarded the English scale, one entirely arbitrary, ascending from the smaller to the higher numbers by wide grades of unequal length. He chooses the French scale, which is perfectly scientific. It takes the millimetre for its unit, and the number of the instrument denotes its circumference in millimetres. Thus No. 1 French is one millimetre in circumference; No. 20 twenty millimetres; and so on. Compared with these, No. 1 English is equal to No. 3 French, and No. 11 English to No. 20 French. Here is a gauge Mayer and Meltzer have made for me, with forty sizes cut in the plate. The plate is marked on one side with the French numerals, and also graduated with a decimetre divided into centimetres and millimetres. On the other side, the English numerals are marked opposite their respective sizes, and there is also a scale of six inches, divided into sixteenths of an inch. Thus the catheter-gauge forms a ready means for comparing French with English measures. Dr. Otis's series of bulbous sounds are spread out before you. They have a slender stem of about four millimetres screwed into the wider end of a bulb or bullet. The best shape for the bulb is that of a turkey's egg, which you know is a little more pointed at the small end than the egg of a common hen. The bulbs range in size from No. 8 to No. 40. Their number corresponds to the big end.

With these instruments Dr. Otis has measured over 500 urethræ, from which he tells us that the expanding capacity of the urethra bears a constant ratio to the circumference of the penis below the glans. Hence if you measure the outside of the penis you can foretell the size of the urethra. Further, that the average size is between thirty-one and thirty-two millimetres, or an inch and a quarter—that is, more than half as large again as the measurement hitherto accepted. From these observations also Dr. Otis finds that the meatus, when not congenitally narrowed or contracted by balanitis in boyhood, a frequent occurrence, is as wide as the rest of the urethra. I have not measured a number of urethræ approaching to 500, but I will give you the results of my measurements so far as they

go. Since last spring I have measured ninety-five urethræ (all of them in subjects of urinary disease by the way), and in only three did I find the meatus as wide as the rest of the canal. One of the three exceptions was that of a man who never had gonorrhœa, but an exceedingly narrow traumatic stricture of the bulbo-membranous part. In him No. 32 sound slid easily down to the stricture by its own weight. This would show that the meatus may be either normally narrower than the rest of the canal, or that morbid contraction is exceedingly common. Be that as it may, practically we have generally to deal with a narrow meatus, the average size being twenty-two millimetres. The measurement of these ninety-five urethræ has satisfied me that we have underestimated the size of the urethra, and that Dr. Otis is correct in claiming large calibres for that canal. But I have not found the bore, so to speak—the capacity for distension, in more accurate diction—to be uniform from the bulb to the meatus. At the bulbous part the urethra is widest, and remains of even width for about two inches. It then narrows gradually, and for the rest of the passage is about three millimetres less, being most narrow at the outlet. This is, as you well know, in agreement with the description of anatomists, only that the distensile capacity of the urethra measured in the living body was greater than the limits they set down. Thus the spongy urethra is conical in shape, resembling the tapering nozzle of a syringe. Whether this be a provision of nature to make the escaping stream more forcible I know not, but you will recollect that the special *raison d'être* of this part of the urethra is to conduct, not the urine, but the semen. Of this I am persuaded, that the less calibre of the urethra at this point is natural, and not the result of chronic inflammation, so long as it is gradual and not abrupt. Morbid narrowings are easily perceived by the sudden way in which they obstruct the bullet, and by the suddenness with which it is released when they are passed.

Next, with regard to the invariable presence of a non-dilatable area of the urethra, a band of contracting fibres—that is, a stricture of more or less development, in every case of gleet. I repeat that I still think that stricture in any shape is not the sole cause of gleet, though doubtless this is the most frequent condition in such cases.

With respect to stricture being most frequent in the first inch and a quarter of the urethra, out of 258 strictures, Dr. Otis found 115 within that distance of the meatus, and the remainder in decreasing frequency in each succeeding inch. This you know, is contrary to the received doctrine, which places stricture most frequently at the bulbo-membranous part. My experience does not support Dr. Otis's statement. In 1870 I

recorded 63 strictures, examined with bulbous sounds, at the Male Lock Hospital in 1869, when I found them 43 times between four inches and a half and six inches—a position, allowing for variation in length of the passages in different persons, almost the same in all.

Next, having ascertained the presence of some unnatural narrowing of the spongy urethra, does internal division cure it, and prevent its return or further development, and consequently cure the gleet? If we adopt Dr. Otis's teaching our course is simple. A patient applies for cure of a gleet. His gleet must be the consequence of stricture. Find that stricture; cut completely through it to the erectile tissue, so as to make the urethra a little wider than before, and take care to maintain this artificial patency while the incision is healing: the cure is then complete and permanent. This is the result of operating in a large number of cases, a report of which has been published in several forms. In 100 published cases, 31 patients were found without recontraction when examined at a considerable period after operation; 52 others were not examined, but reported themselves well; the remaining 17 were not quite cured, though relieved. Such evidence induced me to give a trial to this method. I may state that all the cases operated on here were those of long-standing gleet with contraction in one or more parts of the spongy urethra, and had undergone multifarious treatment. The number of patients is 16; 15 of my own and one of Dr. Otis's—the case on which he operated in our theatre on the 6th of July last. In five cases the gleet stopped after the operation, and the patient was at the last report—taken in none less than three weeks, in most some months, after the operation—able to pass a bougie of the estimated size of the urethra. In short, they may be claimed as cures. But of these five the operation was serious to two; one had free bleeding for three days, the other three attacks of rigors. Of the remaining eleven, among whom Dr. Otis's own operation must be included, the gleet persisted in all; in several the urethra shrank again to its size before the operation, and in some very serious complications ensued. In four bleeding lasted several days, and in one was even alarming. Three patients had rigors; in two the shivering was unimportant, being that which follows the first transit of urine along the incised urethra in certain individuals, but is not repeated or attended by further consequences. In the third patient the rigors preceded abscess in the buttock. One patient had orchitis. Thus in seven the operation might fairly be termed a trifle, causing no pain or any after-fever; but in five only was the operation successful. It may be contended that want of practice on my part, or imperfect performance of the opera-

tion, were the causes of this small success. But I am protected against this danger by having had the benefit of Dr. Otis's personal instructions, and by the fact that one of the least successful cases was that in which Dr. Otis operated himself. The man was in sound health with the exception of his gleet and contraction of the urethra at two and a half inches from the orifice. He made light of the operation, submitting most patiently to the somewhat prolonged manipulations; being animated by the patriotic resolve, as he afterwards told us, that "No Yankee should make *him* flinch." The patient bled copiously after the operation; the hemorrhage not stopping altogether for six days. No other complication ensued, the man was able to get up as soon as the bleeding had stopped, and would have left the hospital at once if permitted. However, though he remained some weeks longer with us, and afterwards attended assiduously for the regular passage of the sound, his gleet persisted till Christmas, and was at last cured by other means. There still remains a scar or induration in the erectile tissue, which gives a crook to the organ during erection. Whether the division of the contracting band caused permanent absorption thereof in any of these patients I do not know, but have very little expectation that it did so. Certainly it failed of this effect in almost all.

When telling you, as those who frequent my wards already know, that I have abandoned this operation for curing gleet accompanying slight contraction of the urethra, I should not omit to tell you that in one point I have varied from Dr. Otis's operation. His urethrotome, which I hold in my hand, is used as follows:—The instrument is passed along the urethra until the end is well beyond the stricture. The instrument is then dilated until it stretches the urethra to its full capacity, or, to make sure, to one or two millimetres beyond that capacity. Next a small cutting edge, previously concealed, at the end of the dilating part, is drawn along the tightly-stretched tissue to the meatus. This long furrow is made in the mesial line in the roof of the urethra. Disliking this long cut, which divides uncontracted parts, as well as the strictured parts, I have employed, except in one case, a stricture incisor, which, while it stretches the urethra to the size previously determined, cuts only where it is strictured. Its mode of action I shall explain when speaking of the division of narrow strictures. As the contracted areas are as freely divided by this plan of cutting, I cannot fairly charge it with the numerous failures that have attended Otis's plan of treating wide strictures.

The plan of treating these affections to which I have returned is that which I adopted before—namely, the repeated passage of bougies, large enough to distend the stricture, but

not large enough to be tightly grasped. The size of the bougie is increased at each visit—that is, about twice a week—to keep pace with the increasing expansion of the urethra until the capacity of the uncontracted parts is reached, when the same size is continued by the patient himself for several weeks longer. When the meatus is greatly smaller than the rest of the passage, I cut it either by Otis's meatome, this straight probe-pointed bistoury, or by a bistouri caché, to which Coxeter has added a second shield, which can be separated from the first by a screw-pin, and so make the fibres tight before they are divided. The incision is made in the floor, and must be pretty complete, as the little ring of fibres is very tough, and often needs two or three applications of the knife to divide it fairly.

But do not misunderstand me. I do not mean that every gleet requires instrumentation forthwith as a matter of course; on the contrary, avoid the use of instruments whenever you have satisfactory evidence that the discharge is not of long standing. The exact length of time that indicates stricture is uncertain, for the inflammatory induration constituting stricture is formed very slowly in some persons, but comparatively fast in others. As a general rule, don't search the canal when the discharge has lasted only six months or less. Be sure, however, that the whole duration of the discharge is really contained in six months, and that there have not been previously periods of clap or gleet to which the present discharge is only a successor. Several relapses of gleet are very strong evidence of stricture. Bear constantly in mind that the introduction of an instrument of any kind into the urethra is an evil, and though in time the canal gets accustomed to the foreign body, this, like many other faculties, is not acquired without discomfort or pain. Resort to instrumentation only when you are satisfied there is legitimate cause for it. Nevertheless, when you do employ instruments to search for stricture, use such as are adapted to the end in view.—*Lancet*, April 8, 1876, p. 522.

64.—ON THE EXTENSION OF INFLAMMATION FROM THE EPIDIDYMIS TO THE URETHRA.

By FURNEAUX JORDAN, Esq., Professor of Surgery to Queen's College, and Surgeon to the Queen's Hospital, Birmingham.

I have had many opportunities of showing cases of epididymitis following urethral disease. The disease is a common one, and its causes are many—almost too many to count. It would be easy to run over some half a hundred, and yet leave a few unmentioned. Urethral disease following and resulting from inflammation of the epididymis is, on the other hand, rare; it has few causes, and is not yet at all recognised.

I show you here a case of urethral discharge, which followed traumatic inflammation of the epididymis. This young man was in good general and genital health. He had never had gonorrhœa. His urethra, meatus, and prepuce were perfectly healthy when he came into the hospital. He received a violent kick on the left testis, and came in two days afterwards with some ecchymosis in the scrotum, and severe pain, tenderness, and swelling, which were clearly seated in the epididymis. On admission, the cord was not thick, nor tender, nor painful. The day after admission, I myself found part of the cord greatly thickened—the thickened portion running up from the testis, and gradually subsiding just below the outer inguinal ring. The second day after admission, the swelling of the cord had passed into the canal, which was now full and tender. Two days later a urethral discharge, neither abundant nor scanty, and whiter than ordinary gonorrhœal fluid, made its appearance, and with it micturition became a little painful and a little more frequent. This is the first case I have been able to show you, and I have myself seen very few.

Recently I saw a case of epididymitis, also from injury, of much interest, and having a direct bearing on our subject. A gentleman, aged fifty, in fair health, came, in the dark, with his right testicle against a key in a door. Scrotal ecchymosis and considerable epididymitis followed. The cord became much enlarged, running like a rope up into the inguinal canal. Later, an abscess formed and broke at the front of the scrotum. Now, in this case, from first to last there was no urethral discharge. There was undoubtedly inflammation of the epididymis, and as undoubtedly the inflammation ran up the cord—probably to the urethra.

The question naturally suggests itself, albeit parenthetically, Why does inflammation of the mucous surface yield a mucopurulent discharge in one case and not in another? Why, in one case of inflammation of the bladder or bronchial tubes, is there abundant pus and mucus, and in another case little or none? I reply, We do not know. You frequently hear me use the expression “We do not know.” I think it better to say plainly we do not know than to cover our ignorance with sounding phrases. It may seem paradoxical to you, perhaps, but I often silently contemplate the benefits of a negative system of surgery—if we could get it: I mean a systematic review of what we do *not* know. The author would be a man of no ordinary courage; he ought to be a man of learning, experience, and judgment equal to his courage.

The case I have just told you proves the conclusion I wish to bring before you—namely, that inflammation may run upwards from the epididymis—more forcibly, in one respect, than

if a discharge had followed. You are aware that at the height of a consecutive epididymitis the urethral discharge often disappears, only to return a little later; but here it was clear we had no hypocrite trying to hide a clap, because there was at no time any clap to hide.

Touching the question of suppuration, I find, with surprise, that so careful an observer as Van Buren denies its occurrence in epididymitis. If there be an abscess, he infers that the body of the testis is inflamed. I believe that in this country surgeons are agreed that suppuration is not rare in the consecutive or other epididymitis of cachectic men.

Let me ask you now to look for a few moments at the causes of inflammation of the epididymis and cord. Bear in mind that any inflammation of the prostatic urethra may extend along the cord to the epididymis. Now, an inflammation of the bladder of any kind, whatever its cause, under whatever circumstances it arises, may run forwards to the prostatic urethra, and thence to the epididymis. Again an inflammation of the anterior urethra of any kind, however caused, arising under whatever circumstances, may run backwards to the prostatic urethra, and thence to the epididymis. Acute inflammation arising primarily in the epididymis has few causes: I can speak positively of one only—injury. If the causes of primary epididymitis were more numerous, we should, I think, long ago have held urethral disease to be one of its results. Why injury sets up epididymitis mostly, and not orchitis, is no doubt for the same reason that inflammation of the cord and epididymis so rarely reaches the testis—namely, that within the tunica albuginea there is no room for acute inflammation, for vascular changes, cell-proliferation, migration of cells, the running riot of germinal matter, or other inflammatory phenomena.

If you accept the facts of the case before you, and my reading of them, you will now and then be able to explain an otherwise obscure gleet. Moreover, you will not be compelled to affirm that in every case an epididymitis and a gleet together have necessarily a urethral, much less a sexual, origin. The views now before you may occasionally have (I speak from personal observation) important domestic or legal bearings.

The treatment I adopt here is that which I am in the habit of adopting in inflammations; but which, seeing that it is exceptional, and strange to examining boards, I rarely take up your time in explaining. My creed is brief: I believe that the inflammatory process is much the same everywhere. I believe that the best remedies in one inflammation are the best in all. I believe that the best remedies are removal of cause (where it is removable), rest, adjacent and not too near counter-irrita-

tion, suitable pressure, elevation, and a few simpler matters. I therefore in each case try to find out the best way of using these remedies. In acute epididymitis I enjoin counter-irritation lightly over the scrotum, sharply in the groin and front of thigh; pressure, which begins as a delicate support, and goes on to actual compression; rest and elevation, as far as they are practicable. The value of compression is well seen in the circumstance to which I drew your attention a moment ago—namely, the pressure kept up on the testis. If our bodies were mapped out into small areas, and each area were bounded by a firm capsule like the tunica albuginea, we should be free from the dangers of at least acute inflammation.—*Medical Times and Gazette*, March 4, 1876, p. 246.

65.—ON THE TREATMENT OF VESICAL IRRITABILITY AND INCONTINENCE OF URINE IN THE FEMALE BY DILATATION OF THE NECK OF THE BLADDER.

By T. PRIDGIN TEALE, Esq., M.A., Surgeon to the General Infirmary at Leeds.

[Mr. Teale has been making observations upon the subject of this essay for a period of eight years.]

About eight years ago the following case, of which unfortunately I cannot find any record, was under my care at the Leeds Infirmary:—A healthy-looking middle-aged woman had been for some time under treatment for great irritation of the bladder, accompanied with the passage of mucus tinged with blood. Having failed to relieve her by the usual means, I decided to dilate the urethra in order that I might introduce the forefinger into the bladder, and search for some explanation of the pain and bloody discharge, such as an ulcer or villous growth. No definite lesion was discovered; but, to my surprise, in the course of a few days, I found that she was completely relieved. She left the hospital, and I have not heard of her since.

This case struck me forcibly as parallel with the immediate relief which generally follows forcible dilatation of the anus for fissure and spasm of the sphincter; and I determined to profit by the hint. The result is, that during the last eight years the operation of dilating the neck of the female bladder as a means of cure of a distressing and unmanageable malady has been adopted in thirty or forty cases by myself and my friends.

The cases calling for the operation are those in which there has existed for some months pain at the neck of the bladder and too great frequency of micturition, with or without pus, mucus, or blood in the urine; or incontinence of urine, with a full bladder unable to empty itself completely, or with a nearly empty bladder which is intolerant of the presence of urine. The time

for doing the operation is when all ordinary means of relief have been tried in vain, whether general or specially directed to the bladder, uterus, rectum, or kidney.

This definition may be said to embrace a very wide range of disorders, having in common the one symptom—irritability of the neck of the bladder. The fact is, the operation has been tried and has succeeded in a great variety of cases. In some successful ones there have been pain and excessive frequency of micturition, giving rise to a suspicion of stone in the bladder, with urine perfectly healthy, and in which, at the time of the operation, the finger failed to detect any fault in the mucous lining of the bladder. In other cases the urine has contained altered pus and mucus and blood, such as is usual where there is vesical calculus, and yet the operation has cured. In others, again, there has been an abundance of unaltered pus, settling in large quantities to the bottom of the vessel, which was afterwards proved to have been derived from a suppurating kidney or other extravescical source; and even in these the operation has given substantial though perhaps temporary relief. More than one case has proved this fact—*that pus from a source external to the bladder may give rise to distressing irritability of the bladder, with pain and excessive frequency of micturition, the bladder itself being apparently sound*; and that the vesical irritability may be cured—for a time, at any rate—by dilatation of the neck of the bladder, although the original cause of the irritability remains in force.

Vesical incontinence.—In two cases (Mr. Scattergood's and Mr. Horsfall's) a perfect cure, and in one of my own cases very great mitigation, of vesical incontinence has resulted from dilatation. In these cases I suspect that there is an over-sensitive state of the mucous membrane at the neck of the bladder, which, the moment an attempt is made to empty the bladder, sets up spasm of the vesical sphincter, and arrests the act of micturition after the escape of a very small quantity of urine, the bladder remaining nearly full and ready in a very few minutes to induce another painful and equally ineffectual attempt at micturition. Thus an overflow of a bladder disabled from correct micturition by irritability of its sphincter becomes an incontinence. Thus also the operation I have proposed by rendering the irritable sphincter vesicæ powerless for spasm, perhaps temporarily incompetent, allows the bladder to empty itself completely, and the cervical mucous membrane to recover its tone.

Operation.—The patient being under the influence of an anæsthetic, Weiss's dilator is introduced into the urethra, and dilatation continued until the urethra is distended so as to admit two fingers. The dilator ought to be passed as far into the

bladder as possible, otherwise the branches of the instrument yield too much to the resisting neck of the bladder. It is desirable to dilate slowly, so as to avoid, *as far as possible*, laceration of the mucous membrane of the urethra. In many cases this laceration occurs, although due care has been taken, and much more after-pain results in consequence during the first day or two.

Sometimes instead of Weiss's dilator I have used the conical probe-gorget, designed by my father for median lithotomy, as a guide for the finger, by which the remainder of the dilatation is effected. On the withdrawal of the dilator it is generally found that the urethra still grasps the finger. The dilatation thus attained enables the surgeon to explore the interior of the bladder. Generally the bladder has appeared to be perfectly healthy, being soft and velvety to the touch throughout. Occasionally portions have been raised and indurated, as if ulcerated, but I have thought this sensation might be deceptive, and produced by the corrugations of an empty bladder. I cannot say that I have undoubtedly detected ulceration by the finger in any of these cases. In two or three instances it has turned out that the bladder was permanently contracted to a cavity little larger than a thimble.

[It may be objected to this operation that there is danger of producing incontinence of urine. This, however, is not the case, on the contrary, "it has happened that the operation was rarely followed by any incontinence of urine, and many of the most rapid and most complete cases had not even temporary incontinence. On the contrary, instead of relieving the bladder every few minutes or every hour, they were able to retain their urine the greater part of the night immediately following the operation. Other cases, again, had incontinence of urine for a few weeks. One case for a few months had very little power of retaining urine and then recovered completely, the pain and irritability having been relieved from the first. Two cases that I know of have suffered from permanent incontinence of urine. It does not, however, appear that the liability to permanent incontinence depended upon the degree to which the dilatation was carried."—*Lancet*, Nov. 27, 1875, p. 764.

66.—ON DILATATION OF THE FEMALE URETHRA.

By CHRISTOPHER HEATH, Esq.

Mr. Teale's recent paper on the Treatment of Vesical Irritability in the Female by Dilatation of the Urethra, shows that he and I have unconsciously been working in parallel grooves for some time. I was not aware of Mr. Teale's practice when, in 1871, I adopted a very similar proceeding at the Hospital

for Women, Soho-square, to which institution I was then surgeon. Failing to cure cases of painful micturition by other measures, I adopted the plan of rapid dilatation of the urethra with the finger guided by a director, in some cases using a pair of ordinary polypus forceps to commence the dilatation. My idea was that, in some cases, there was a fissure of the mucous membrane analogous to fissure of the anus, but this I was never able to prove, though the rapid dilatation almost invariably produced a tear in the subpubic portion of the mucous membrane, which might, in some cases, be the continuation of a fissure. In a course of lectures on Diseases of the Female Urinary Organs, which I delivered at Soho-square in February, 1873, I narrated many cases illustrating this practice, and from the number of American surgeons who at that time visited the hospital and saw my practice, I imagine that the introduction of the practice into America is due to my example. I can fully confirm Mr. Teale as to the benefit conferred on many cases by the treatment and its failure in others, and I have never seen permanent incontinence result, though my patients have often been unable to retain the urine for twenty-four hours. I have, however, never pushed the dilatation quite to the extent practised by Mr. Teale, having been content to take one finger as my limit.

In another direction, however, I have pushed on somewhat beyond Mr. Teale, and that is in the application of topical treatment to the female bladder. In cases of chronic cystitis, with purulent urine, I have detected, as I believe, an ulcerated condition of the mucous membrane (for with the left forefinger introduced into the bladder it is easy with the other hand to bring the whole of the mucous membrane into contact with it), and I have then applied a strong solution of nitrate of silver by means of a mop, securely fastened on a wire, and passed through a small vulcanite speculum introduced into the urethra. The effect of this has been, in many cases, to render the urine acid and clear in twenty-four hours, and the tolerance of the remedy by the bladder is remarkable. The solution I have employed has been as strong as three drachms to the ounce, and I have never seen any harm follow even where little permanent good was done.—*Lancet*, Dec. 11, 1875, p. 858.

67.—IRRITABILITY OF THE FEMALE BLADDER OF FIFTEEN YEARS' STANDING CURED BY DILATATION OF THE URETHRA AND NECK OF THE BLADDER.

By H. BENDELACK HEWETSON, Esq., Leeds.

[The case related in the following paper is of great value as bearing direct testimony to the value of dilatation of the

urethra in the female for the relief of irritability of the bladder. This curious effect of dilatation in some cases was discovered accidentally by Mr. Pridgin Teale of Leeds, he having dilated simply for the purpose of exploring the bladder; nothing was found, but the irritability was cured.

Mr. Hewetson observes:—"The absolute success attending upon its performance in some cases, and its partial, if not complete, failure in others, render it incumbent on us to keep strict records of the symptoms, general as well as local, which affect females the subjects of irritable bladder upon whom this operation is performed. The extent to which the dilatation of the urethra is carried should especially be noted, since the partial or complete failures may possibly be the result of too cautious stretching from fear of producing incontinence of urine. Moreover, the general symptoms must be taken into account, for it may be that it is upon them that the surgeon is consulted, without the slightest reference being made (as this case will show) to the more delicate point of irritability of the bladder; when it is upon the latter trouble that the general malaise depends."]

Following are the notes of the case:—

Miss M., aged thirty-six, sent for me on the night of March 3rd, 1875. On arrival at her home, I found her to be suffering intensely from retention of urine. I relieved her (by the use of the catheter) of a large quantity of urine, such an amount as must have distended the bladder nearly if not quite up to the umbilicus. This point I did not test, being anxious to relieve her without delay from the worst agony of retention. The retention returned in a day or two, and I had again recourse to the use of the catheter.

Her previous history is as follows:—That she was a perfectly strong and robust woman, following the *arduous* duties of a "present day" schoolmistress until fifteen years ago, when she was seized with an inflammation of the bladder, for such it was termed by her medical attendant. This was followed by the formation of a small abscess in the region of the urethra, which discharged of itself. Since that time her health has been bad, preventing the continuance of her calling. She states that since her recovery from this attack, her nights have been wakeful and disturbed by being constantly "every half hour or hour" obliged to get up to pass small quantities of urine with great effort and some pain. She is low and depressed, with almost constant headache, loss of appetite, and continual bearing down, resulting in a total unfitness for prolonged exertion of any kind. She has lost flesh considerably, and has for several years been set down by her numerous friends and medical advisers, to whom she had made no

reference to her urinary troubles, as a confirmed invalid. These symptoms were complicated a year ago by pain in passing the motions, which were infrequent and confined. She had not been seized by retention of urine previous to March last. In February last she was, with "questionable propriety," advised to come to Leeds to stay with some friends for the good of her health.

On examination I found the orifice of the urethra to be completely surrounded by warty growths of a considerable size, and on examining the rectum the introduction of the finger was impeded by a very tight sphincter ani. The rectum was baggy, and there was a small external pile. The uterus was in its natural position, and the catamenia were, and had always been, regular, and the urine was natural. Not being able to estimate to what extent the retention was due to the warty growths, and seeing she was suffering considerably from rectal difficulties, I deemed it advisable to negative the possibility of the retention being caused by the warty growths by removing them, and whilst the patient was under the influence of an anæsthetic stretching the sphincter ani with the forefingers introduced back to back sufficiently forcibly to paralyse it for a time and allow the sore produced by the snipping off of the pile to heal; in the same way as one would cure a fissure of the anus, by setting the spasm of the sphincter ani at rest, which, constantly contracting, might possibly be an element, through reflex action, in helping to keep up the vesical irritability.

Accordingly, on March 5th, chloroform having been administered, I completely carried out the above suggestions. The result of this was, that during the next few weeks relief was given to the retention of urine and to the pain in passing the motions; but there was no relief at all from the vesical irritability. Her general condition, with these exceptions, continued as before, and there was a return of the retention of urine at the end of three weeks from the operation.

I had clearly told her a second operation would possibly be necessary should the first fail to give relief, and accordingly, on April 11th, I again placed her under the influence of chloroform, and introducing Weiss's female dilator into the urethra to the extent of about two inches, I then *slowly* separated the blades of the dilator, stretching the urethra so as to admit of the introduction of my forefingers within the bladder while the parts were on the stretch. On closing the blades and withdrawing the instrument, the urethra contracted upon my little finger, so as sensibly to grip it when introduced into the bladder, the coats of which were thickened. There was no foreign body or stone to be detected.

She was very much upset by the chloroform-sickness, which continued more or less all night; there was, however, *no more irritability of the bladder, no retention, no incontinence produced*, and, to use her own words, "I have not passed water so freely for years;" nor had she retained it so long without being disturbed, for the first time she made water was in the evening after the operation, and she was not disturbed during the whole night.

April 14th. She slept the night through, and awoke with little or no headache, retaining or passing her water quite naturally, with the exception of some soreness.

16th. She complains of slight pain while passing water, but perfect facility.

19th. She got up in the evening complaining of none of her former symptoms, having lost completely all trace of vesical irritability in eight days from the dilatation of the urethra.

Her progress now continued to be satisfactory and very rapid.

May 4th. She reports her old symptoms to have all disappeared. She sleeps and eats well, and takes a fair amount of exercise.

At this stage I ordered her into the country, where she resides, and returning on May 31st, she reported herself as perfectly well, and has gained three stones in weight. On the day previously she had walked a distance of eight miles without feeling more than ordinary fatigue, and remarked that before the operation "she could hardly trail herself about."

Thus, then, were the miserable and intractable sufferings of years, shutting out this poor woman alike from society and employment, put an end to at once by an operation whose best recommendation is its simplicity and its success.—*Lancet*, Dec. 4, 1875, p. 796.

68.—ON DILATATION OF THE FEMALE URETHRA.

By Dr. ARTHUR W. EDIS, Assistant Obstetric Physician,
Middlesex Hospital.

The clinical essay of Mr. Teale, the remarkable case cited by Mr. Hewetson in the *Lancet* of Dec. 4th, and the experience of Mr. Heath, recorded in the *Lancet* of Dec. 11th, encourage me to publish the notes of two cases that may possibly prove of interest to the profession, as illustrating the necessity of caution in adopting this expedient, and also the importance of excluding other possible causes of irritable bladder before resorting empirically to forcible dilatation of the urethra. I have notes of a large number in my case-book, but select the following as illustrative of many others. The conditions most frequently

producing irritation at the neck of the bladder, with painful or frequent micturition, apart from any alteration in the quality or quantity of the urine itself, are chiefly vascular growths in the urethra, cystitis, stone in the bladder, passage of small renal calculi, pressure upon the neck of the bladder from an ante-flexed fundus uteri, dragging down of the posterior wall of the vagina (as in cases of prolapsus uteri), fissures or cracks in the urethra, extension of malignant disease from the uterus, or malignant degeneration of the bladder itself.

Case 1.—M. D., aged forty-two, married, sterile, first consulted me in December, 1872, for irritability of the bladder. Micturition was frequent and attended by scalding. The symptoms had come on gradually during the last month or six weeks, and had lately much increased in severity, the patient being obliged to get up several times during the night to pass water.

Examination of the urine detected nothing abnormal beyond the presence of lithates. On examining per vaginam, there was no evidence of any unusual discharge. The vagina was moist and cool; no vascularity of the urethra was apparent; the uterus was normal in bulk and position. No local lesion likely to account for the symptoms was found. On exploring the bladder by means of a sound, no calculus was felt. The only unusual symptom was soreness of the urethral passage, but, as the patient was very nervous, it was thought at the time to be nothing calling for further attention.

Saline aperients with diuretics and restriction of the diet were enjoined, and the patient urged to resist the desire to micturate so frequently.

As the irritability of the bladder and soreness on micturition persisted, a pair of leeching forceps, about the size of an ordinary quill, was inserted, and the blades forcibly dilated so as to distend the urethra. The little finger being then passed, roughness of the posterior wall of the urethra was detected, but as the operation caused much pain nothing further was done. Later on in the day I received an urgent summons to visit the patient, and found her very weak and faint, lying in a pool of blood and urine. On making a local investigation a distinct jet of bright blood was seen after removing some clots and employing the catheter.

A No. 10 elastic catheter was inserted and left *in situ*, pressure being applied along the course of the urethra by means of cotton wool inserted per vaginam. This arrested the hemorrhage for the time, but on removing the catheter the following morning, the bleeding recurred. A sound coated with cotton wool and saturated with solution of perchloride of iron was passed per urethram, which again arrested the hemorrhage for

a time, but it was not until a No. 12 elastic catheter covered with cotton wool steeped in tincture of matico was passed and retained *in situ* for twelve hours that the hemorrhage was finally arrested.

The case caused me much trouble and anxiety, and impressed me forcibly with the risk incurred in resorting to forcible dilatation of the urethra.

The patient convalesced rapidly, regained perfect control of the sphincter within the course of a few days, and had no further symptoms of irritation.

Case 2.—E. H., aged eighteen, single, servant, presented herself as an out-patient, complaining of frequency of micturition. During the day she stated that she could only hold her water for about half an hour, and was obliged to get up at least five or six times during the night to empty the bladder. This condition had been gradually coming on during the last two years, and was always worse about the time of her periods, but had been much worse the last six months. Iron, belladonna, bromide of potassium, chloral, ergot, strychnia, and every other likely drug were given at different times, without more than the most temporary benefit. As the condition remained unaltered, she was admitted as an in-patient at the Hospital for Women.

A careful vaginal examination showed that the uterus was anteflexed, though not to a very marked degree. Buchu, uva ursi, triticum repens, alkalies, and acids gave no relief. Confinement to bed, restriction in the amount of fluids, blisters to the suprapubic region, the adjustment of an india-rubber ball inflated with air per vaginam so as to compress the urethra, injection of two-grain doses of morphia into the bladder, and every other available expedient was tried without avail. At this time Mr. Heath saw her in consultation, and suggested rapid dilatation of the urethra. Chloroform was administered and the operation performed; the neck of the bladder being brushed over with a strong solution of nitrate of silver. The symptoms remained unchanged. The urine was passed between five-and-thirty and forty times during the twenty-four hours (as ascertained by the patient pricking a card with a pin every time she made water).

The case now seemed hopeless. The patient's condition was most deplorable. The frequent necessity for getting up during the cold winter nights, the discomfort from constantly wetting her linen, and the offensive ammoniacal odour produced rendered her existence almost unbearable.

Every method of relieving the bladder having been tried in vain, I thought, before discharging her as incurable, it would be well to try the effect of an intra-uterine stem. After some

preliminary passing of the uterine sound, a small vulcanite stem was inserted, but was extruded within a few hours. Having made one with a bulbous extremity this was inserted and was retained *in situ*. Within a few days she had perfect control over her bladder, and before leaving the hospital some ten days after the insertion of the stem, she could pass the whole night without getting up, and only micturated four or five times in place of forty during the twenty-four hours. She took a place and remained perfectly well, until one day, hurrying to catch a train, the stem came out, when all the old symptoms recurred. She was obliged to come up to town and have it reinserted, and has done so twice during the last two years. During the present month she has written to say how very grateful she is for the relief, and that she remains quite well.—*Lancet*, Dec. 25, 1875, p. 909.

69.—THE RAPID DILATATION OF THE URETHRA IN WOMEN.

By Professor SPIEGELBERG, Berlin.

Prof. Spiegelberg remarks that the great dilatability of the urethra in women has long been known. Large stones have been extracted or passed from the bladder; and in cases of masturbation the urethra has been so much dilated that intercourse has been effected through it. Its dilatation for various reasons has often been practised by the surgeon. Its rapid dilatation is, however, of recent date. It is very painful, and can therefore only be undertaken after the administration of anæsthetics. Either the finger or specula, specially designed for this purpose, may be employed after the external orifice has been incised (Simon). The operation is completed in a few seconds; the bleeding that accompanies it is slight and always free from danger. At most it is only sufficient to interfere with the ocular, but never with the digital, examination. The canal may be enlarged by this means almost to the diameter of an inch without danger. Incontinence of urine is a rare subsequent event. Spiegelberg adopted this measure with great success in the treatment of a case of cramp of the bladder. A woman, aged 24, who had had a child a year previously, consulted him for difficulty of micturition, which first made its appearance after her confinement, at first experienced only at intervals, but subsequently more frequently, whilst it had become more troublesome. The ordinary means of hip-baths, &c., had proved useless, and a ring, introduced for a presumed anteversion, only produced great irritation and discomfort. Evacuation of urine was performed every half hour, was very painful, in jets, and accompanied by tenesmus. The patient was much

depressed. The vulva and urethral opening were reddened. The projection into the vagina of the urethra thickened, the urine bright and clear, the introduction of a canula very painful, and after its withdrawal a little blood escaped, uterus normal, no stone. Spiegelberg suspected the presence of a small polypus in the upper part of the urethra or neck of the bladder. He performed sudden dilatation with a Busch's dilator. On introducing a Tobert's intra-uterine speculum, he saw at the upper and narrowest part of the urethra an elongated sore and granulating surface about half an inch long, which did not bleed. He touched it with nitrate of silver, and complete recovery followed. A case of fissure of the neck of the bladder was treated in the same way, and made a rapid recovery. Spiegelberg associates these cases with fissura ani, which also frequently occurs after confinements, and in which forcible dilatation is a sovereign remedy. There are a number of cases of so-called cystitis and urethritis and of neurosis, with painful contraction of the bladder, resembling vaginismus, in which forcible dilatation proves the best means of cure. In one case Spiegelberg discovered, by means of rapid dilatation, a polypus in the lower part of the bladder, which had been for years the cause of trouble, and in the same way he punctured a case of hæmatometra not to be reached per vaginam.—*Practitioner*, Dec. 1875, p. 448.

AFFECTIONS OF THE EYE AND EAR.

70.—CLINICAL LECTURE ON A CASE OF CATARACT EXTRACTION.

By Dr. CHARLES BELL TAYLOR, Surgeon to the Nottingham and Midland Eye Infirmary.

Having decided upon associating an iridectomy with the operation of extraction for cataract, the question arises, shall we do iridectomy now and extract later, or shall we remove the lens and a portion of iris at one and the same time? I should certainly much prefer to make the iridectomy a separate operation, and extract some weeks hence when all trace of the operation, such as haziness of the cornea at the site of the wound, or congestion of the neighbouring tissues, had passed away. By adopting this course we obtain much valuable information as to the probable conduct of the patient when submitted to the major operation. We ascertain how anæsthetics are borne; whether they occasion much vomiting, struggling, or excessive congestion; or whether the patient has sufficient steadiness to bear the operation without anæsthe-

tics: and it is clear that we may utilise the experience thus obtained to the benefit of both patient and operator when extracting at a later date. The formation of an artificial pupil is in itself but a trifling operation; and a subsequent extraction, when the parts have quite recovered, is much simplified by the previous removal of a small portion of the iris: whereas if the iridectomy is made a part of the operation we have a fresh wound of the iris, corresponding in size to the margin of the coloboma, in addition to the one made in the cornea for extraction; in fact the amount of traumatism, or actual injury, at the time of operation is increased, and it was formerly considered nothing less than a disaster to accidentally slice a piece of iris while doing an extraction. The excision of a portion of iris which follows the first incision is also often accompanied by a gush of blood which fills the anterior chamber, and obscures the subsequent steps of the operation. The raw edges of the recently-wounded iris are apt to be contused during the passage of the lens, and if consequently inflamed, they will often adhere to any remaining portions of the capsule, and contribute to the formation of a secondary cataract. Certainly a preliminary iridectomy is for many reasons the safest procedure. Mooren, of Düsseldorf, to whom we are indebted for the suggestion, operated thus in 62 exceptionally unfavourable cases with two failures only; and some years ago I published an account of 100 similar cases, treated by the same method, with equally good results. But there are unfortunately insuperable objections to Mooren's method which are well illustrated by the case now under consideration. This patient, for instance, cannot afford to stay here for eight weeks; if we do an iridectomy and send her home, she will conclude, so soon as she finds that her sight is not improved, that the operation has been a failure—an opinion that is sure to be loudly endorsed by numerous acquaintances. She would be horrified at the idea of a second operation in the face of such a failure, and we should probably never see her again, even if she were in a position to incur the expense. We must therefore do the best we can under the circumstances, and proceed to extract, making the iridectomy a part of the operation. Shall we do so by the upper or lower section? By the upper section the coloboma iridis is in many patients concealed by the upper lid; and there is evidence to show that the process of healing is to some slight but not very important extent facilitated by this position of the wound. These are the arguments which are used in favour of the upper section; and some of our best operators, notably Mr. Streatfield, of Moorfields, invariably adopt it; but if suppuration should occur, and suppuration is imminent in this case, the downward gravitation of pus and other inflammatory products will form a

most unfavourable feature in the subsequent progress of the case. The eye is always turned pretty firmly upwards during anæsthesia, and involuntary, without anæsthetics, by the patient; to turn it downwards with forceps in order to expose the upper segment of the cornea involves the use of a speculum, and is attended with some risk of rupture of the hyaloid membrane, and immediate loss of the eye. The upward tendency of the eyeball, and the downward gravitation of inflammatory products are, on the contrary, rather fortunate incidents if the lower section be adopted; and although I have operated successfully in a large number of cases by the upper section, I think there can be no question that the lens, the capsule, and remnants of cortex are much more easily extruded through an inferior incision: and, contrary to the generally received notion, I have convinced myself that the inconvenience from a coloboma in this situation, especially if it be a small one, is not in excess of one made directly upwards, while the sight is equally good, if not better; this is no doubt owing to the fact that the downward position of the eyeball when reading, or gazing at an object, while exposing an upward iridectomy brings the one made downwards under the protection of the lower lid. We shall therefore extract by the lower section, and make the iridectomy a part of the operation. With regard to operating upon both eyes at once, of course it is better to do one at a time; but here again we are often driven by the patient's mental condition, or domestic circumstances, to do that which, if not absolutely the best, is the best under the circumstances. For instance, if I operate upon one eye of this patient, and succeed, she will hardly be inclined to risk the other: if I fail she will be too discouraged to do so; so that I think it will be well to multiply our chances of success by a double venture.

The only question remaining to be considered before we proceed to operation has reference to the employment of anæsthetics. I should much prefer to dispense with anæsthetics in the operation of extraction, on account of the vomiting, congestion, and struggling which they often occasion; but there are many cases, and this is one of them, where the patient's want of self-control is such as to preclude the possibility of operating without first inducing insensibility. If it were not for vomiting there would be but slight objection to the use of anæsthetics, and vomiting ceases to be of much consequence if a piece of iris has been removed, although the repeated straining is very apt to occasion prolapse if the pupil has been left entire. What anæsthetic shall we employ? Nitrous oxide is safe, and scarcely ever causes vomiting, but it occasions struggling, spasm, and a kind of epileptic condition, so that insensibility can

seldom be conveniently maintained until the operation is completed: chloroform, bichloride of methylene, and methylene ether are pleasant to inhale, but they all cause vomiting,—that from chloroform sometimes lasting for twenty-four hours,—while the patient is with each of them exposed to the risk of fatal syncope, or death beginning at the heart. Ether is the only available anæsthetic, with which I am acquainted, that is free from this special risk: this constitutes the real superiority of ether as an anæsthetic.

You may well say, if iridectomy is so valuable a resource why not resort to it in every case of extraction? The answer is simply this:—"It has its disadvantages, and is not always necessary; a patient with a large artificial pupil, and no power of accommodation, is apt to suffer from dazzling cosmetic defect, and other inconveniences, which although to be regarded lightly,—considering the paramount importance of restoring light, are, nevertheless, defects which we ought to avoid in all favourable, or even average cases. It would be quite superfluous to remove a portion of iris in moderately healthy patients, with mature cataracts, and in cases where the operation has been completed without misadventure; however desirable it may be to do so in those where these conditions do not exist. In numerous recent cases I have extracted with the same form of incision without mutilating the pupil, and the results have been most brilliant, some of the patients declaring that they saw with glasses not only as well but even better than ever in their lives before; and we can readily understand that, in the absence of other defects, a perfect crystal may more than compensate for the absence of a natural lens which perhaps was never a very brilliant one: in fact there can be no doubt that the iris plays a most important part after extraction; regulating the amount of light admitted to the eye, correcting the aberration occasioned by sphericity of the cornea, diminishing irregular circles of diffusion; contributing to the clearness of excentric vision, and thus after extraction compensating largely for the absence of the lens. The great danger if we extract without iridectomy is prolapse of the iris. This risk is however much diminished by the instillation of a solution of esserine, or calabar bean, immediately after the operation. Some surgeons hesitate to employ this agent on the ground that if iritis should occur we should have the unfavourable complication of iritis plus a contracted pupil, but in point of fact primary iritis does not come on until the third or fourth day, not until the wound made for extraction has, in the great majority of cases, healed; and the effect of calabar bean may then, if necessary, be completely neutralised and overcome by the instillation of atropine."

I should, as a rule, keep the eyes bound up for a week, and confine the patient to a couch for three or four days. I find the best dressing is a little cotton wool, fine charpie, or French wadding, dipped in a solution of chloral hydrate: this disinfects any secretion which may accumulate between the lids, or rather prevents the decomposition which might prove the starting point for suppuration of the wound. The wool is best retained *in situ* by one turn of a domett bandage pinned over the forehead so as to afford slight support to the eye, and thus maintain the edges of the wound in apposition.

This is readily unpinned and turned back, without disturbing the patient, whenever it is necessary to inspect the eye or change the dressing. About eight hours after the operation the edges of the lids should be gently sponged with an antiseptic lotion, just barely separated, and any slight secretion removed. As a rule patients do not have any pain, but if pain should come on after operation it is always a serious symptom, and should be combated *ab initio*. If accompanied by a full pulse, swollen lids, and coated tongue, I generally take blood from the arm, or foot, to the amount of four ounces or more if the patient will bear it, and administer a calomel purge, with a subcutaneous injection of morphia, and apply a compress bandage, with hot fomentations. This was Von Græfe's practice, who assured me, when I attended his clinique in Berlin, that he had learned, by sad experience, never to employ leeches or abstract blood from the immediate vicinity of the eyeball for the first three days after an extraction. In a recent case in which I adopted this treatment, the eye was saved and the patient has now excellent sight, although suppuration had actually set in. After thirty hours the danger of suppuration has, as a rule, passed by, and after the third day any pain the patient may suffer is probably due to primary iritis, which may be detected on inspection, and which is best treated by atropine, morphia, blue pill, and the application of four leeches to the temple, a fresh one being added as fast as one drops off, until the pain is gone.—*Practitioner*, December 1875, p. 404.

71.—SECTION OF THE CORNEA IN CERTAIN DISEASES OF THE EYE OF INFLAMMATORY ORIGIN:

ABSCCESS OF THE CORNEA: OPHTHALMITIS: IRIDO-CHOROIDITIS SYMPATHETIC (?): WITH A NOTE ON THE TREATMENT OF HYPOPION BY CILIARY INCISION.

By J. VOSE SOLOMON, Esq., Professor of Ophthalmic Surgery in Queen's College, and Surgeon to the Eye Hospital, Birmingham.

Some years since, the late Mr. Guthrie, senior, advised the treatment of large collections of pus in the anterior chamber by

a vertical and central incision extending from one side of the cornea to the other, and that a Beer's cataract-knife should be used for the operation. Soon after reading his paper, which was, I believe, about twenty years ago, I tried the plan in a single instance, but was not satisfied with the result; the failure, I am now persuaded, was due to the greatly disorganised state of the eyeball—in fact, to the operation having been too long deferred.

For the last thirteen years it has been my practice to evacuate extensive collections of pus or puro-lymph occurring in persons of feeble constitutional power, and in whom some layers of the cornea had sloughed, or evinced a tendency thereto, by making a radiating incision through the corneo-scleral union and the ciliary sclerotic for a distance of about two lines. There is no method with which I am acquainted more safe or effective; and it has the additional advantage of depleting the engorged ciliary choroid. It was from observing in colliers and labourers who had passed their fortieth year, that a peripheral semi-lunar corneal incision was commonly followed by destruction of the membrane and staphyloma, that led me to confine my puncture almost exclusively to the sclerotic. My colleagues Mr. Bracey and Mr. Owen, who, from having been formerly house-surgeons of our hospital, have had peculiarly favourable opportunities of judging, and of comparing this method with others, follow the same practice, and are well satisfied with its results.

In the course of 1873 and 1874, I treated successfully by Guthrie's operation (vertical incision), with some limitation as to the length of the wound, several cases of extensive *hypopyon* complicated by abscess, or by diffuse infiltration of the cornea; conditions which were sometimes coexistent in the same eye. The tendency of the infiltration to extend into the adjacent healthy tissue was not always at once checked; consequently, superficial scarifications, punctures, and even a second deep and rather free incision, made at an angle with the line of the first wound, by giving early and facile exit to the pent-up purulent matter, were employed with advantage. In all cases it is superfluous to state that preventable causes of debility, as, for instance, over-suckling, should as a matter of course be removed.

In the administration of quinine I generally prefer to give half or a whole grain every hour to a large dose at intervals of four hours; in this way, I believe, the occurrence of suppuration in an organ may be sometimes lessened in extent, if not averted.

Whatever operation is performed, the warm water pad proves for the first few days a valuable local application. Drops of atropine and nitrate of silver are also useful where the local reparative power is defective.

The good effects of treatment by vertical incision may be summarised as—relief from pain, which is *not* always immediate; the preservation of a more perfect curve of the cornea than where the membrane has suffered ulcerative perforation, and consequently a less degree of astigmatism; frequently the prevention of synechia anterior, and staphyloma; also a less extent of any leucoma which may follow the presence of an onyx.

I now come to the chief and almost sole object of making this communication to the Association, namely, *the application of free section of the cornea to cases of deep seated ophthalmia threatening destruction of the eye*. I am indebted to our intelligent house-surgeon Mr. Eales for many of the notes of the two following suggestive and instructive cases.

Case 1.—A little girl, aged 12 months, was admitted under my care at the Birmingham Eye Hospital, on August 20th, 1874, with acute photophobia and muco-purulent conjunctivitis of the right eye; the cornea was throughout densely opaque. The child, always delicate, had suffered from diarrhoea for three weeks immediately prior to the appearance of the ophthalmia. On the ninth day of admission, the conjunctival discharge had ceased, but protophobia, with severe pain, which had deprived the child of rest and sleep from the time of admission, continued. At this time, an examination made under chloroform revealed great congestion of the conjunctiva and sclerotic, especially in the ciliary region; the cornea was so gorged with lymph that no iris was visible. A little above the transverse diameter were noted two small rough eminences consisting of lymph, probably points from which pus had escaped. Tension + 1. The aspect of the eye was most unfavourable. While examining it, the possible usefulness of a free incision of the cornea as a preventive of sloughing of the part and a relief to the tension suggested itself, and at once I carried the idea into effect; making the wound not vertically but somewhat horizontally through the more dense part of the corneal infiltration, and on a level with the upper border of the pupil. The relief was rapid; sleep at once returned, and the day restlessness ceased. The whole periphery of the cornea began to clear. On September 15th, the twenty-sixth day of treatment, the patient returned home to Evesham.

On November 23rd, Mr. Eales made the following notes. "The iris can be plainly seen all round, the cornea being merely nebulous at this part. In the centre and the immediate neighbourhood of the line of incision, the leucoma is dense. No relapse of ophthalmia since the incision was made. No staphyloma, no synechia." The child was too young to admit of the state of the retina being tested.

The results which followed operation in this case suggest the possibility of curtailment of the duration of some cases of keratitis by a resort to section of the part most deeply infiltrated, and so allowing of escape of the exudation *inclosed* in the proper substance of the cornea. That the eye was saved from destruction by the special treatment adopted can scarcely admit of doubt.

The next case presents features of still deeper interest to the ophthalmic surgeon.

Case 2.—Irido-choroiditis of the Right Eye, and Collapse of the Left Globe from Sloughing of the Cornea consequent upon Scarlatina Aginosa.—Edith Rogers, aged 7, was admitted into the Birmingham Eye Hospital, August 31st, with acute irido-choroiditis of the right eye and collapse of the left, attended by great irritability and acute conjunctivitis on both sides. The child was in low health, having suffered at Christmas 1873 from scarlet fever, which was followed in two months by dropsy of the hands and feet. The ophthalmia commenced about a week after the onset of the fever, and the left became in three weeks blind from sloughing of the cornea. Inquiries from the mother elicited that she could read six weeks after the fever, but that it “injured the eyes.” On admission, the state of the ocular tissues was as follows. In the right eye, great congestion of the external tunics, and of the choroid veins; iris greenish, devoid of lustre, and rather woolly on its surface; the pupil black, circular, and contracted to a diameter of one-twelfth of an inch, insensible to the mydriatic effect of atropia; the cornea clear. Tension—1 to 2. The left globe was collapsed, its conjunctiva tumid and highly inflamed. Intense photophobia affected both eyes. The patient, it was said, had never complained of pain.

Treatment.—The inflamed stump was excised, with some temporary benefit to the right eye.

On October 12th, six weeks after admission, finding the state of the right eye and its vision were in no degree different to what they were before excision was performed, I considered the proposal of a resort to some surgical operation with a view to arrest the persistent irido-choroiditis. Iridectomy was dismissed as inapplicable, from the probability of being followed either by a destructive ophthalmitis, or closure of the enlarged pupil by an irido-capsulitis. Peritomy I determined to keep as a reserve in case of the failure of the proceeding that was adopted; which consisted in the making, during the anæsthesia of chloroform, a vertical incision in the cornea, at mid-way between the outer side of the pupillary border and the corneo-scleral epiphysis. The extremities of the wound were by design not extended to the margin of the cornea, as I con-

ceived that the process of repair set up in such a wound would be more likely to relieve the cyclitis than if the incision terminated near to the roots of the iris. The eye was dressed with the hot-water pad. On the seventh day of the operation, a marked retrocession was observed in all the objective symptoms. The iris was bright, but still greenish in colour; the conjunctiva and sclera uninflamed; the latter presented a few dilated and tortuous veins upon its surface. Tension— $1\frac{1}{2}$.

The vision improved so much that the child could guide herself and pick up pins from the floor of the day-room. Her health also underwent a change for the better. The state of the vision in the course of the two months the patient remained in the hospital after the date of operation varied at uncertain intervals, and occasionally a patch of redness would appear on the scleral conjunctiva, and be attended by undue sensitiveness to light, but there was no return of the iritis. On Dec. 14th, she returned home. The iris was bright, but greenish in colour; the pupil clear and immobile; some scleral injection was present, and irritability to light. Vision about her usual average; she counted fingers at twelve inches. Tension— $1\frac{1}{2}$.

In the first of these cases, the destruction of the cornea and formation of a large staphyloma were imminent, and were unquestionably prevented by free incision of the infiltrated and gorged membrane.

The second is an example of an extremely rare sequela of scarlatina, unless we view the irido-choroiditis as bearing the relationship of sympathetic ophthalmia to the inflamed stump. I do not pretend to make even an attempt to decide so intricate a question, but content myself with drawing the attention of surgeons to the evident safety of the plan adopted in a class of cases which has hitherto proved little, if at all amenable, to any kind of treatment; and I would fain hope that surgery has acquired, in free section of the cornea, an addition to her scant and imperfect armoury for doing battle with one of the most destructive internal inflammations of the eye.—*British Medical Journal*, March 18, 1876, p. 343.

72.—EXAMINATION OF THE EYE-BALL BY LATERAL OR OBLIQUE ILLUMINATION—OPHTHALMO-MICROSCOPE.

By HAYNES WALTON, Esq., Ophthalmic Surgeon to St. Mary's Hospital.

It is seldom that students are taught to examine the surface of the eyeball, a matter of instruction little inferior to teaching how to examine with the ophthalmoscope. It is my intention

to show you how this is to be done by what is called lateral illumination.

This method, then, of lateral illumination, is effected by incident light. It is particularly suitable for examining the anterior part of the eyeball, both without and within. By brilliant illumination of this kind objects which might be overlooked by the diffused daylight are clearly discerned, and diagnosis is furthered. The cornea, the iris, the pupillary margin, the capsule of the lens, the ciliary processes, and even the front of the vitreous humour can thus be inspected. The impaction of minute extraneous substances in the cornea, and corneal nebulæ, are rendered very apparent by it. The difference between phlyctenular and interstitial corneitis is made more palpable. Plastic deposits on the capsule of the lens are well shown. The nature of a cataract is more easily determined by this than by any other mode of examination. The consistence of the cortex of the opaque lens and the colour and density of the nucleus can be told only by it. Opacities in the vitreous humour that are solid and always appear black when examined with the ophthalmoscope are now seen, when sufficiently anterior and small, in their natural colours and in their proper positions, and the inequalities of their surfaces and other physical peculiarities can be estimated.

Examination by Artificial Light.—A darkened room is better for a beginner; one that is partially illuminated will suffice for an observer who is experienced in the method.

The patient is placed, sitting or standing; the bright flame of a lamp is arranged at the side, on a level and a little behind his head. The observer takes his position in front of him, and holds a bi-convex lens of three, two and a half, or two inches focal value, in such a manner and at such a position as to receive the lamp rays, and to condense or focus them on the region of the eyeball to be examined.

The fullest amount of illumination is got by placing the patient close to the lamp and using a very high lens.

Any special point to be examined must be placed exactly under the focus of the lens.

For examining the crystalline lens the degree of obliquity of the cone of rays must be less, as the equatorial portion is inspected, and greater as the pole is looked at.

In examining the vitreous body the rays must be thrown in the greatest obliquity. The observer should be prepared to find the interior of the eyeball no longer black, but purple or smoky.

The object examined may be magnified by the observer using a convex lens, the selection of which should depend on the nature of his own state of sight.

Mr. Walton now gave a series of demonstrations on patients in illustration of his discourse.

Examination by Daylight.—The lateral illumination by natural light is not new. I saw it in use when I was a student. After the above description it is unnecessary to say how this is to be done. It is enough to remind the reader that the patient should be placed near a window, and in as strong a diffused light as can be got.

Dr. Heddaens has strongly advocated the use of this form of examination. He uses a pencil of rays admitted through a central perforator, from one to two inches in diameter, in a screen sufficiently large to shield the head of the patient from diffused light. The pencil is concentrated by a lens in the usual way. The doctor claims for it the advantages of showing objects in more natural colour, of easy application, and an absence of discomfort to patient and observer. He avoids sunshine, and finds the diffused light even of a cloudy day sufficient for all purposes.

Ophthalmic-Microscope.—This is merely a common hand microscope, with which an examination of the object is made by reflected light, and for the most part employed with lateral illumination of the eye. The chief use of the instrument is for minute clinico-pathological research in the living eye, for the better seeing of corneal deposits, minute vessels in the cornea, minute changes in the iris, and in the lens. For practical purposes it possesses no advantages over the simple microscope or lens. The instrument may be used as a fixture, and screwed to the table for the purpose of steadiness. That of the kind most known has arrangements by which it rests on the patient's face. Such an one is delineated in Wecker's "*Etudes Ophthalmologiques.*"—*Med. Press and Circular*, Dec. 29, 1875, p. 529.

73.—ON CONJUNCTIVAL TRANSPLANTATION FROM THE RABBIT TO THE HUMAN SUBJECT.

By Dr. J. R. WOLFE, Surgeon to the Glasgow Ophthalmic Institution, and Lecturer on Ophthalmic Medicine and Surgery in Anderson's University.

For a number of years I have practised an operation for the radical cure of symblepharon, by taking a portion of the neighbouring healthy conjunctiva of the eyeball wherever I can get it, and supplying the palpebral conjunctiva which has been destroyed; for the loss of conjunctiva oculi, even to a very large extent, is generally regenerated without prejudice to the neighbouring tissue, whilst loss of the conjunctiva of the eyelid is sure to damage the cornea by friction. Mr. Teale had first introduced this operation, which I practised independently with considerable modifications.

But there is a limit to the extent to which we may go in borrowing conjunctival substance from an eye which has been already injured; if we surpass that limit, the corneal opacity is sure to extend still further, and we lose the last chance of saving vision. Indeed, we sometimes meet with cases in which the infliction of a new wound (in peeling off conjunctiva) would amount to destruction of the eye. It is in these desperate, I would say, almost hopeless, cases that I have devised the method of making up the deficiency by transplanting a portion of the conjunctiva from the rabbit. This method has since been practised by von Wecker, Otto Becker, Albrech Graefe in cases of symblepharon; and Professor Cohn has shown a case in which he successfully transplanted the conjunctiva from the rabbit into the right eye of a boy five years old, for the cure of the deformity resulting from the removal of a large melanotic tumour of the eyeball.

The method having thus had some years' trial by such competent authorities, I shall not enter here upon the theoretical vindication of the procedure, but merely show you two patients upon whom the operation has recently been performed, which will enable you to judge for yourselves of the utter hopelessness of any other method, and of the satisfactory result obtained by this.

Case 1.—F. B., aged nine, had his right eye burned with lime in January, 1875, producing adhesion of the lower eyelid to the eyeball to the extent of covering the greater portion of the cornea, the cornea being thus rendered almost entirely opaque, with the exception of the small upper and outer segment. On lifting the upper eyelid the pupil was seen covered by the adherent lower eyelid, and there was no conjunctiva of the eyeball left available for supplying the deficiency. I therefore first turned the transparent portion of the cornea to account by making an artificial pupil upwards and outwards, whereby I restored a considerable amount of sight. Having procured this advantage, I operated ten days ago for the cure of the symblepharon, by transplantation from the rabbit. I would request the gentlemen present to take a careful view of this patient. You will see that some of the ligatures are still in the new conjunctiva from the rabbit, and on seizing the loose portion of this new membrane with the forceps, it will show you clearly beyond any possibility of mistake, the vitality of the transplanted portion; you will see that same membrane has some greyish points here and there, while the rest of its surface has assumed a shining pinkish appearance. You will see also the mobility of the eyeball, and the action of the eyelid restored.

Case 2.—P. C., aged 25, quarryman, received an injury in his face and eyes from a gunpowder explosion in January, 1872.

When he first came to the Glasgow Ophthalmic Institution, four months after the accident, his face was quite riddled and discoloured by powder; both upper and lower eyelids of the right eye were considerably everted, and the left eye was completely closed and blinded by symblepharon. I made an iridectomy upwards, which resulted in tolerably good sight when the upper eyelid was raised. On presenting himself in January, 1873, I remedied the symblepharon by transplantation from the rabbit.

In July, 1875, he came for the treatment of the everted eyelids of the right eye, when I found that a small bridge had formed in the new conjunctiva, which did not mar the result of the operation, this being, indeed, his best eye. However, I separated the bridge, and supplied a new conjunctival flap from the rabbit a fortnight ago.

The examination of this eye will greatly aid us in the appreciation of the method. The extent of the corneal opacity in this case will show the amount of the original injury, which time has considerably repaired. We can also compare here, in the same eye, both the old flap inserted about two years ago and the new small bridge supplied only a fortnight ago, and judge of the satisfactory result.

Now, I will proceed to give a detailed account of this operation.

I generally put both the patient and the rabbit under chloroform. I then separate the adhesions so that the eyeball can move in every direction. Next I mark the boundary of the portion of the conjunctiva of the rabbit which I wish to transplant by inserting four black silk ligatures, which I secure with a knot, leaving the needles attached; these black ligatures indicate also the epithelial surface, which would be very difficult to distinguish after separation. I take from the rabbit that portion of the conjunctiva which lines the inner angle, covering the membrana nictitans, and extending as far as the cornea. I select this on account of its vascularity and looseness. The ligatures being put on the stretch, I separate the conjunctiva to be removed with scissors, and I transfer it quickly to replace the lost conjunctiva palpebræ of the patient, securing it in its place by means of the same needles, and adding other two stitches, or more, if requisite. Both eyes are then covered with a bandage and dry lint. For the first forty-eight hours the conjunctiva has a greyish look, but it gradually loses that appearance, and, with the exception of some isolated patches here and there, it becomes glistening, in some parts looking not unlike conjunctival thickening. These patches gradually decrease until the whole assumes a red appearance.

Should any irritation set in, I apply warm-water fomenta-

tions. With regard to chloroform, Professor Cohn operates on the rabbit without anæsthetics, and I should myself prefer to do so if I could manage satisfactorily, for rabbits do not stand long anæsthesia; but the struggles of the animal are a very great obstacle in the way of the careful dissection required. With regard to the patient, it must be borne in mind that it is a long and tedious operation, taking about three-quarters of an hour in its performance; and that under no circumstances is it safe to maintain insensibility throughout the whole process. In a grown-up intelligent person, therefore, I do it without chloroform; and in the case of the young boy referred to, I put him under chloroform only during the first stage—namely, while separating and dissecting the adhesions,—and completed the rest by coaxing the patient into submission.

Accidents during the operation.—It is always desirable to have at least two rabbits at hand in case one should succumb under chloroform. I may mention also that in one of the operations, after putting the ligatures on the stretch and dissecting the portion of the conjunctiva which I marked out for removal, two of the ligatures with half of the conjunctiva tore away from the rest. To give up this eye and proceed to operate on the other eye of the animal would not have been advisable, on account of the time required to do so; I therefore determined to proceed with the operation in the following manner:—I spread out the portion of the conjunctiva thus removed upon the back of my left hand, and the needles and ligatures attached to it I held in the palm of the same hand, and proceeded to remove the rest of the conjunctiva from the rabbit, and, by means of the remaining two ligatures, I fixed this on the conjunctiva palpebræ. This accomplished, I turned to that portion of the conjunctiva spread out on my hand, which I found dry and firmly adherent to the hand; a few drops of warm water put upon it sufficed to soften it and make it relinquish its hold of the skin, and I then transferred it to its proper destination.

Von Wecker puts the conjunctiva upon a watch-glass, and warm water underneath; but I consider the process accompanied with many inconveniences, and I would never resort to it myself except in a case of emergency. I consider quick transplantation, without previous handling and preparing that delicate membrane, decidedly preferable.

The needles which I employ are half-curved No. 12, which Weiss makes for the purpose. Two small silver spatulæ are also requisite for spreading out the conjunctiva. There are various kinds of needle-holders, but I find my own fingers most suitable for the purpose.

In conclusion: although, as stated before, I have resorted

to this method only in hopeless cases, when there was no disposable neighbouring conjunctiva of the burnt eye for supplying the deficiency, I found the result so highly satisfactory that under no circumstances would I think myself justified in removing healthy conjunctiva from an injured eye; but in all cases I would supply the deficiency from the conjunctiva of the rabbit, as being the safest and best method now at our disposal for the cure of such injuries.—*Lancet*, April 8, 1876, p. 526.

74.—ON A NEW METHOD OF MAKING PERVIOUS THE
EUSTACHIAN TUBE AND OF INFLATING
THE TYMPANUM.

By Professor Dr. JOSEPH GRUBER, Superintendent of the
Clinique for Aural Patients in, and Aural-Surgeon to,
the Imperial General Hospital of Vienna.

[There is often great difficulty in catheterising the Eustachian tube from obstructed conditions of the nasal meatus, and from the nervousness and timidity of the patient. There are very few men who have had any great experience in the operation.]

One of the chief aims that we commonly have in view in passing a catheter is to blow air through the Eustachian tube into the cavity of the middle ear—a procedure which is briefly designated as the air-bath, and the employment of which in aural surgery, as is pretty well known, is, of all the means of treatment that we as yet are aware of, that which is the most often indicated and the most frequently used. And, in short, it is consequently desirable that we should know of some means of giving the air-bath, which would permit of our attaining the same, or at least approximatively the same end, without the introduction of the catheter into the Eustachian tube.

The method which has of late years been employed with much success for this purpose is that which was first described by A. Politzer. This, as is well known, is as follows:—The patient having taken a small quantity of water in his mouth, the end of the nozzle-piece of one of the indiarubber bags, which Leiter (the instrument-maker) has specially constructed for the purpose, is passed about half an inch up the nose; the nostrils are next hermetically closed on the nozzle-piece by means of the thumb and forefinger of the operator's left hand, and then during the swallowing of the water, which the patient has to perform at the command of the operator, the latter, holding the bag in his right hand, suddenly compresses it.

According to the generally entertained views on the subject, the upper part of the pharynx at the moment of the act of swallowing is by the contraction of the structures of the throat

completely cut off from the lower portion of the pharynx, and at the same time the tuba Eustachii opens, whereby it happens that the air compressed in the nasal cavity, being prevented from escaping in other directions, passes through the tube into the cavity of the middle ear.

In spite of the very valuable gain which practice has derived from this method, it nevertheless presents many drawbacks, to which we shall refer later on, when we have described the new method.

Lucae more recently, on the basis of observations on the function of the soft palate and the Eustachian tube, which observations he had the opportunity of making by direct inspection of the functional parts in the case of a patient who presented an extensive deficiency of the nose, recommended that for the inflation of the tympanum the nozzle-piece of the bag should be introduced into the nose just as in Politzer's method, and that while the patient intoned the vowel *a* the bag should be compressed. During the phonation of the vowel *a* the soft palate becomes tense and shuts off the upper pharynx, and then air can be forced, as in Politzer's method, through the tube into the tympanum.

I purpose referring again in another place to these labours of the distinguished author, and will here only briefly mention that I have convinced myself that it is possible in many cases to drive some air into the tympanum in the manner described, and that, as the author himself specially pointed out, this method may be useful in the treatment of small children.

In the treatment of adults, namely, when it is a question, as in such cases it is apt almost always to be, of overcoming a stronger resistance in the tube, the method recommended by Lucae is certainly not easy of accomplishment. For in the case of morbid changes in the tube—and with such we have chiefly to deal—the air entering the pharynx quite easily overcomes the obstacle offered to it by the slight tension of the soft palate during the phonation of the vowel *a*, and finds vent downwards without getting through the Eustachian tube into the tympanum.

In order that the air may enter the tube with the requisite force, it is indispensably necessary that the closure of the upper pharynx should be such as is capable of offering a strong resistance; and to attain this in a simple manner a mere swallowing movement, or some movement analogous to it, is sufficient; and since in the employment of the swallowing movement, as in Politzer's method, I have found many disadvantages, I thought of an analogous one, and I believe I have found a better one for our purpose.

I will now proceed to describe it, giving at the same time the

description of the method which I shall recommend in association with it.

It relates, as I have often mentioned, to the best possible closure of the upper from the lower pharynx, in which the muscles of the soft palate and co-ordinately the muscles of the tube shall enter into action. All this is attained by simply pressing the root of the tongue against the hindermost segment of the palate, if at the same time a powerful expiration is performed. In pressing the hinder segment of the tongue against the palate the cavity of the mouth is cut off from the throat, and at the same time the soft palate is pressed upwards and backwards. The air which, owing to the simultaneous expiration, enters the throat, finds egress neither through the mouth nor through the nose—a fact which may be readily proved by holding before the nose at the same moment either one's hand or a small flame. The flame is not disturbed even in the slightest degree, and the hand does not perceive the least sensation of a breath of air during the expiration, which, however, it certainly would if air issued from the nose. The stronger the expiration, the more tense is the soft palate rendered by the air that passes from the lungs into the throat, and so much the more resistant is the closure of the upper pharynx. This moment, on account of the position of the structures of the throat, would be the fittest for imparting the greatest possible force to air forced out of a bag inserted in the nostril.

If we always had perfectly tractable patients to deal with, we would certainly employ this position. However, as this is not the case, we must be contented with such movements as approach as nearly as possible those just described, and these, according to the studies which I have made as to this point on myself and on patients, are those which are caused when we pronounce the consonant *hck* (= *hkk*) in the most accentuated manner. Since, however, even with respect to this, it is necessary to have regard to the varying intelligence of patients, and since it is not always a matter of the firmest closing of the upper pharynx, and since it is easier for the surgeon if he directs the patient to repeat after him a full syllable, he can intercalate a vowel as in *hack*, *heck*, *hick*, *hock*, *huck*.

If we pronounce the above-named syllables in the order here written down, we may convince ourselves that the tongue becomes drawn further backwards and more strongly upwards as we proceed with the list of syllables, so that with the syllable *hack* the tongue is most forward, at the syllable *huck* it is pushed furthest back to the upper part, and that in like proportion the upper pharynx is contracted and forcibly shut off. The closure is still more forcible, and carried further backwards, by the pronunciation of the consonants *hck* than it is by the syllable *huck*.

We have therefore in this series a gradation, or, as it were, a scale of closure of the upper pharynx, which, as we shall see later on, is very useful.

As the reader has already noticed, the syllables are always written with *ck* (or equally *kk*), to which I need scarcely add that the *k* must be strongly accentuated. In proportion as the speaker may please to strengthen this emphasis, so is he thereby, and indeed very considerably, enabled to close firmly at his pleasure the upper pharynx.

If we now attempt to pronounce one of these syllables we notice peculiar changes in our ears. We remark each time a distinct movement in the membrana tympani, and by no means unfrequently a noise resembling that produced by the Valsalvian experiment. It consequently follows that in this experiment also the air must enter by the tube into the tympanic cavity, and that the tube during this experiment is opened.

The details of the method which, based on all these data, I now recommend for the above-mentioned and other analogous purposes are as follows:—

The operator stands, or sits at his convenience, face to face with the patient. The end of the nozzle-piece of a rubber bag, which the operator grasps in his hand, is introduced into the inferior nasal meatus of the patient for about half an inch, and the operator at once hermetically closes the nostrils on the nozzle-piece of the bag with the fore and middle finger of his other hand, and while the patient at the word of command pronounces one of the before-named syllables (*hack, heck, hick, hock, huck, hck*) the bag is squeezed. Thereupon the air passes with a distinctly perceptible noise through the tube into the cavity of the tympanum.

The noise which is produced by the forcible entry of the air through the tube can be auscultated with precision by means of the otoscope. In cases where the membrana tympani is perforated, the well known perforation sound is produced. The patient feels a distinct sensation of the entry of the air into the tympanic cavity. The membrana tympani exhibits, on inspection, quite distinctly the well known appearances—in short, all the signs of an entry of air into the middle ear having taken place are present, provided the tube is not impermeable in such a degree as to resist also other modes of procedure.

If, during this procedure, I cause the patient to incline his head strongly towards one shoulder, it almost always happens, especially when I insert the bag into that nostril which corresponds to the ear in which I desire to drive the air, that the air flows into the ear directed upwards—that is to say, the one that is turned away from the shoulder. In cases where the air

also entered the other ear, it nevertheless almost always happened that the patient felt the air more strongly in the upward turned ear. If the patient with this kind of procedure had once confidentially declared that he felt the air in one ear, he had also the same sensation if I repeated the trial, either several times in succession or on different days. Whether in this particular position the muscles of one side possibly act more energetically, or whether the forced-in air more readily follows the upward direction, or whether in this position the expired air exercises a stronger pressure on the one half of the soft palate, and so shows the way to the air driven in through the nose, I am unable to say. I would also make these latter statements with all reserve, and only point them out since it would obviously be of inestimable value if we had a means of this kind by which we might be able to drive air through only one tube. To sum up, the advantages possessed by this process as compared with Politzer's are as follows:—

First, it is much simpler, the swallowing motion being no longer required. There is no swallowing of water either. To appreciate this boon, one ought to be acquainted with the unspeakable loathing patients, as a rule, evince for drinking the water, although I have practised this process much less frequently than others. A large number of glasses used to stand in readiness on my consulting table, and yet patients used to be compelled to make an almost superhuman effort just to sip water out of my glass. Indeed, we cannot well blame them for this if we take into consideration that a practitioner's consultation-room must be accessible to all patients, let the nature of their diseases be as contagious as it may.

Apart from this, however, if the process is to be repeated more frequently, the swallowing of water becomes not only troublesome, but actually painful. To the objection that the patient might make a swallowing motion even without water, I reply that in such cases the driving-in of air through the tubes proves often unsuccessful, and that the repeated unsubstantial swallowing is even more painful than the swallowing of water. We will say nothing of cases in which, owing to concomitant throat diseases, the swallowing is painful; neither need we dwell upon the fact that when such an attempt is made the air not unfrequently gets into the stomach, producing violent pain, which does not subside until the stomach has discharged this air by repeated eructation.

2. By keeping the throat structures for a more protracted period in the position of the ending of the syllable (*i.e.*, of *k*)) one is enabled to let the air pass somewhat longer through the tubes into the cavity of the tympanum, which is perfectly impracticable during the short act of swallowing, which, as it is well known, cannot be protracted.

3. Taking the above scale into consideration, the air may be pushed with any amount of power at choice through the tubes; and this advantage is to be valued all the more that we know that, owing to the rapidity with which Politzer's process—if it is to succeed at all—must be put into operation, the membrane of the tympanum has often been ruptured.

4. This process, if applied to self-treatment, must needs be much more advantageous, since the patient, guided by his own feeling, may intensify at will the pressure of the air entering the cavity of the tympanum.

I now beg to recommend this process to my highly esteemed *confrères*, hoping that they also will make their experience on the subject public. — *Medical Times and Gazette*, Jan. 1, 1876, p. 7.

AFFECTIONS OF THE SKIN.

75.—ON THE TREATMENT OF CHRONIC ECZEMA BY GLYCEROLE OF SUBACETATE OF LEAD.

By Dr. BALMANNO SQUIRE, Surgeon to the British Hospital for Diseases of the Skin.

[The remedy proposed by Dr. Squire is new to therapeutics, and the view suggested by its use as to the nature of eczema is contrary to that usually entertained. This view is that the affection is a local, and not a constitutional one, and may be successfully treated by local remedies only.]

Many of the profession are well content to employ local remedies in that disease, and even to place considerable reliance on their instrumentality: of such local remedies, not to enter into wide detail, by far the most popular in this country is the benzoated oxide of zinc ointment. I take it that more of this ointment is prescribed in the treatment of chronic disease of the skin within the United Kingdom than of any other kind of ointment. I feel sure that most practitioners have used a larger quantity of this ointment in the treatment of chronic eczema than they have of any other local application. Of late years, however, this popular application has had a rival in the unguentum diachyli, which has been adopted from Vienna—a dilution of diachylon-plaster, with olive oil to bring it to the consistency of ointment. Neither of these applications would have become so generally used unless they possessed some efficacy. I have tried them both very often indeed, and I quite know them to be useful remedies. However, long before diachylon ointment had become popular over here,—certainly long before I myself knew of it,—I had endeavoured to find something more efficient than the zinc oint-

ment, which I had often found to fail of anything like efficiency, and I thought I had found this in lead ointment: I accordingly tried the acetate of lead ointment of the Pharmacopœia—that is to say, a mixture of the powdered crystals with lard. I soon, however, had reason to think, on trial, a mixture of the subacetatis plumbi liquor (Goulard's extract) with lard a more efficient application by far. At length, however, I took to using glycerine in place of lard as a vehicle for applying that remedy. I had previously tried water, in fact, lead lotion of various strengths. Lead lotion of moderate strength (3 ss. ad ʒj.), used as an evaporating lotion, is, indeed, in my experience, one of the most excellent applications for acute eczema; but in chronic eczema I have found it of very little service. Here, lead ointment becomes much more valuable. I venture to say that I feel sure, from ample experience, that lead is an infinitely more suitable agent in the local treatment of eczema than zinc. Lead, however, has been but little used in this country for the purpose. The agent that has been, and still is, chiefly in vogue for this purpose is zinc. It would not be untrue to assert that for some time past—how far back that time may reach I am unable to say—the two principal prescriptions for skin disease in this country have been zinc ointment and zinc lotion. As to the zinc ointment, it is a tradition that it must be *benzoated* zinc ointment. It is probably not generally known to prescribers that this stipulation is a very immaterial one, and from one point of view even a disadvantage. The addition of the benzoin is simply to preserve the ointment (somewhat imperfectly, however) from becoming rancid when stale; and this lame safeguard is apt to induce the pharmacist to keep it (since it is much in demand) in stock, whereas ointments of any kind should always be made fresh, and prescribed in such moderate quantity that they may always be expended rapidly, for no artificial preservation of them will enable them to bear comparison with the fresh preparation. As to the zinc lotion, it has been the fashion to colour it with calamine, and the same kind of tradition has preserved the erroneous belief that the calamine is in some sort an essential therapeutical ingredient of the lotion. Lead, however, is, I am well persuaded, by far a more soothing, and at the same time a much more astringent application than oxide of zinc. I have found that it unquestionably allays the itching, restrains the discharge, and diminishes the hyperæmia of eczema; in short, it *cures* it far more speedily than zinc does. It is of the best mode of applying this efficient agent in the treatment of chronic eczema that I have now more particularly to speak. As a lotion it fails, and I think it is not difficult to see how this happens. The lotion

fails to keep the surface moist: the warmth of the body speedily causes it to evaporate: that is to say, the water passes off as vapour, and the lead is left on the surface of the comparatively dry gummy layer of viscid exudation which is characteristic of eczema. Thus the remedy is precluded very considerably from influencing the skin itself, and is deposited simply on the surface of the scab, however minutely dotted and however thin that scab may be.

Lead ointment is far more efficient. It preserves the moisture of the secretion, and so permits of the passage by osmosis of the remedy from the ointment through the secretion to the surface of the skin.

I have already declared my experience to be in favour of ointment made with Goulard's extract, in preference to the Pharmacopœial preparation made with the powdered crystals of the solid acetate, but I must also declare my experience to be very greatly in favour of either of the above to the Vienne preparation (recently imported) of diluted diachylon plaster. The latter is, to begin with, a very uninviting, putty-like substance, quite like putty softened by admixture with oil.

I have found that for the majority of cases of chronic eczema greasy preparations of any kind are *pro tanto* a disadvantage: they appear to act in a minor degree in the same disadvantageous manner that water-dressing covered by oilskin does. I was accordingly induced, when I had fairly satisfied myself that lead was a good agent, and that the basic acetate of lead was a good form of exhibiting that remedy, to seek some more favourable vehicle for exhibiting it in cases of chronic eczema than grease of any kind had seemed to me to be (I had tried, amongst other vehicles, oil). Glycerine has proved in my hands far superior either to oil or water as a vehicle for applying remedies in the case of chronic eczema. It has neither, so to speak, the desiccating effect of water nor the macerating effect of grease. Its viscosity is about parallel with that of the viscid secretion proper to eczema, so that, in smearing it over, it is readily miscible with it, and while it preserves the surface moist, it readily incorporates itself, and, more than that, undergoes true admixture or diffusion (osmosis) with the viscid aqueous solution of lymph and albumen which is exuded slowly but continuously from the surface of the diseased skin. Thus it conveys the remedy held in solution by it more freely to the actual surface of the skin itself. At the same time it fulfils an indication which is less perfectly attainable by the use of ointment: it adheres more closely to the comparatively moist surface of eczematous skin than ointment does, and so is less readily rubbed off by accidental friction. In this way it more

efficiently preserves a uniform suppleness of the skin affected by eczema—a matter of importance in more than one respect, for not only is the patient relieved from the hide-bound feeling which is one of the chief distresses of chronic eczema, but the chapping and cracking of the skin, caused by the partial splitting on every movement of the body of the inelastic varnish formed on its surface by the partially dried-up exudation, is avoided: and this latter phenomenon is one of the main aggravators and perpetuators of the eruption.

That glycerine possesses ample capacity for penetrating serous secretion may be proved by repeating an experiment which I have often had occasion to make in prosecuting microscopical researches into the minute anatomy of the morbid skin. I have been in the habit often of preserving small portions of skin (removed by operation) in glycerine, so as to preserve them from putrefaction till I had leisure to examine them. It will be found that in a short time a piece of skin so immersed has undergone throughout the whole of its thickness a very marked change in its physical characters. It has lost its whiteness and its opacity, and become of a light yellowish-brown colour and semi-transparent, assuming the aspect but not acquiring quite the hardness of a piece of dried tendon; at the same time it has become curled up at the corners. It feels very considerably harder than before. If cut with a knife, it “cuts” something like slightly warm glue or like an old and somewhat hardened piece of candied orange-peel, which, on the whole, it rather resembles; at the same time it has notably diminished in size. In fact, the glycerine has almost completely deprived it of all its water, which has become diffused throughout the glycerine, while the glycerine itself has thereby become accurately applied to every nook and crevice of the piece of skin in the closest possible contact. In the living skin, of course, even when it is completely raw, desiccation of this kind does not take place on the application to it of glycerine, because the skin is constantly fed, on its under surface, by an inexhaustible supply of aqueous fluid; but it is clear that an agent which has so obvious a capacity for absorbing watery fluids into its substance must be far more capable of bringing medicaments already dissolved in it into much closer juxtaposition with a serum-secreting surface of skin than it is at all possible to attain by applying to the skin the same medicaments incorporated with grease, whether in the shape of lard or oil. Grease of any kind, so far from absorbing watery fluids into its substance, is proverbial for resenting any attempts at its admixture with them. “But, then,” it might be urged, “why not use water? water surely is miscible enough with aqueous solutions; whether serous

or other, the process of osmosis would surely convey the water, and with it the medicament it may contain, into the substance of the viscid film of serum, and so to the surface of the skin itself." This is true; but then, as I have said before, dessication rapidly takes place, both of the lotion and of the serum, and so an impermeable film of dried scab becomes (fatally to the purpose in view) interposed between the skin and the remedy. "Then, why not," it might be urged, "cover the lotion applied on rag with a piece of oiled silk? *that* would prevent dessication." To this the reply is simple—the experiment has been tried often enough, and I have had frequent occasion to note the result: such a plan invariably aggravates the disease very markedly; and more than that. For example, if the patch of wet rag and oiled silk be larger than the patch of disease over which it is applied, it leads rapidly to an extension of the disease. Thus, I have often seen a large round patch of chronic eczema very speedily converted into a much larger square patch, the size and shape of the larger patch, corresponding very accurately to the dimensions of the piece of oil-silk which had been kept applied. The skin becomes thoroughly macerated; and this condition evidently is more prejudicial to a favourable issue in chronic eczema than even its condition as it exists when it is left to itself. Unlike a mere aqueous solution, a solution of the remedy in glycerine permits of its continuous application, while at the same time the skin is preserved supple and *moderately* moist. Unlike an aqueous solution reinforced by a covering of oil-silk, a solution in glycerine permits of a certain degree of continuous evaporation from its surface of aqueous vapour. In short, glycerine appears to have the property of supplying efficiently the most important of the physiological actions of the lost epidermis—namely, of conducting in like manner with the epidermis the business of the "insensible perspiration." When spread out in a film on the raw skin, it fulfils the same office that the epidermis does, in continuously absorbing water on its under surface, and permitting its constant escape from its upper surface in the shape of aqueous vapour, but in such moderate quantity as shall serve to keep the skin itself fairly and properly moist. In addition to this useful quality may be added another. Very many patients have an extreme dislike to being smeared over any considerable portion of their surface with grease of any kind; not only does it make their clothes in a mess—which to a person of cleanly habits is often extremely distasteful—but it induces a peculiarly uncomfortable sensation which is a source of annoyance. Whereas glycerine, even when applied freely over the greater portion of the entire surface, is a comparatively cleanly application, and excites no other sensation than a feel-

ing of slight stickiness, which is far more readily put up with, and is eagerly preferred to the unpleasantness of a general inunction. Even this slight disadvantage completely disappears when, as should always be the case, the glycerole is not profusely applied, but is smeared on in very moderate quantity at each application, and renewed as often as may be demanded—generally about three times a day.

[After some further remarks Dr. Squire describes the pharmaceutical details.]

The remedy is manufactured—or rather, I should say, the stock which forms the basis from which the remedy is prescribed is made—in precisely the same way as the “*plumbi subacetatis liquor*” of the British Pharmacopœia, only that glycerine is used in its manufacture instead of water; and to this I have to add that some simple directions as to temperature have also to be followed. It is thus prepared: Take of acetate of lead, 5; litharge, $3\frac{1}{2}$; glycerine, 20. Heat for half an hour in a boiling glycerine bath, constantly stirring, and filter in a gas-oven or other kind of heated compartment. The result is a perfectly clear and colourless liquid, of a somewhat more viscid consistency than pure glycerine. Physically, it is a stronger solution than the Pharmacopœia “*plumbi subacetatis liquor*,” inasmuch as the insoluble white residue left by the Pharmacopœial process is nearly double by weight the residue left by the process above directed. Therapeutically, however, the glycerine preparation is a far less active application considered absolutely than the “*liquor*,” besides that it differs markedly in several essential respects in the *kind* of effect produced by it. It does not appear to be known that equal strengths of solution in water on the one hand, and in glycerine on the other, of any remedy used locally, are very different as to the degree of effect exerted on the skin, the aqueous solution being in every case a far stronger application than the glycerine solution. The reason of this is very obvious: the rapid evaporation of the water under the influence of the heat of the body very soon concentrates the aqueous solution; whereas the comparatively fixed character of glycerine does not allow of any notable evaporation of the solution.

Two of the pharmaceutical details of the preparation of the glycerole demand a little explanation. The ingredients are not directed to be boiled over a naked flame, because, in boiling, glycerine undergoes decomposition, evolving acid fumes. On the other hand, they are not directed to be heated in a boiling-water bath, because, when thus warmed, less of the powder is dissolved than when they are heated in a boiling-glycerine bath.

Filtration in a hot chamber is recommended, because at the

ordinary temperature it would probably take about a week to filter even a small quantity of the solution, whereas when heated the solution temporarily loses its viscosity, and speedily runs through the filtering paper.

I have called the preparation I have above described the "stock" from which the remedy is prescribed, because as above prepared it is far too strong for general use, although it may occasionally, in certain cases, after one's way has been carefully felt by the previous use of milder strengths of solution, be with advantage used undiluted. I find that a drachm of it to the ounce of pure glycerine is about the strength which is usually expedient. Sometimes half a drachm to the ounce is quite sufficient. On the other hand, two drachms to the ounce will sufficiently often produce a better effect than one drachm to the ounce will; and greater strengths progressively employed up to the full strength will more rarely be found expedient. The skilful use of this remedy, simple as is its composition, is like the skilful use of a very simple instrument. Some people can achieve wonders in wood-carving with a penknife: some cannot. To determine at sight what conditions are favourable to its employment, and what are not, is one matter; to pronounce what strength of it should be used is another, and perhaps a less easy affair, but it is of no less importance as concerns the result. Too weak an application will do scarcely the least good: too strong a one will do positive harm. Practical experience in the use of the remedy will alone suffice to impart judgment in these matters. But these remarks apply with equal force to every local application of any kind that may be employed in the treatment of skin diseases. I introduce them here merely to explain why I believe that any attempt at minuter detail than I have already entered into on this head would probably be an idle one.—*Medical Times and Gazette*, March 18 and 25, 1876, pp. 302, 329.

76.—ON GLYCEROLE OF SUBACETATE OF LEAD IN CHRONIC ECZEMA.

By Dr. J. HANCOCKE WATHEN, Fishguard.

[Referring to Dr. Squire's interesting papers on this subject, given in the preceding article, Dr. Wathen says that his father and himself had been in the habit of using lead in the form of an ointment made with the subacetate liquor, in chronic eczema as well as other skin affections, for the last thirty-five years. He then goes on to say :—]

Dr. Squire has dealt so thoroughly with the subject that hardly anything remains for me to add, beyond offering my

heartly testimony to the efficacy of the treatment he advocates. There are, however, some points of pharmaceutical difference between us which require notice, and, if not too wearying to my readers, I will describe the mode of preparing the ointment as we have generally used it.

After discussing the value of lead ointment made with the subacetate liquor and lard, Dr. Squire announced his preference for the combination of the liquor with glycerine. Of this combination alone I have no experience, as the preparation we use is that of lard, glycerine, and the subacetate liquor, which is prepared in the following manner:—Into one ounce of best clarified lard two or three drachms of glycerine is rubbed. After this has been incorporated the addition of the liquor is commenced; the lead is rubbed in until the lard becomes saturated and will not receive any more. In the manner and care with which this is done consists the whole secret of the success of this preparation.

If great care be taken to coax and tease the liquor plumbi in, as much as one ounce of the latter can be incorporated with the lard before it becomes saturated. This strength is at variance with what Dr. Squire states as the maximum amount of the liquor—viz., one drachm—that can be mixed with the lard. This may possibly be accounted for by the presence of the glycerine, which should be added *before* the liquor, as, if it (the glycerine) be mixed *after* the addition of the lead, the latter will all be driven out. As I have written above, much depends on the patience exhibited in rubbing in the liquor. By pressing and lifting the spatula from the surface of the lard, a number of cells are produced; over these the lead is poured, and gently rubbed in with the lightest of hands, and with a little skill a full fluid-ounce of the liquor should be taken up by a similar weight of lard. Some lards take up more than others; and the weather has an influence: if the weather be cold and frosty, the lard will take up a proportionately smaller quantity.

In the various states of chronic eczema of the moist type, as defined by Dr. Balmano Squire in his paper, the above ointment has been found of the highest value after other treatment has signally failed. While, on the one hand (as Dr. Squire has already pointed out), by the application of Goulard's lotion to an eczematous surface the diseased action can be extended *ad infinitum*; on the other, by the use of the ung. plumbi subacetatis (liquor), or (according to Dr. Squire) by the glycerole of the subacetate of lead, we have an expeditious and efficacious method of curing these eczematous affections. In applying it to the limbs, the better method is by spreading a number of strips of bandage with the ointment, and using the strips after the many-tailed plan. This gives support to the congested

bloodvessels, and insures more perfect contact of the ointment with the diseased surfaces. I can thoroughly endorse Dr. Squire's statement as to the freedom from risk of lead-poisoning. Never has a single case occurred to me of the faintest suspicion of absorption of the lead into the system, although a considerable amount of surface has been enveloped in ointment—for instance, the whole of both legs, from knee down to the foot; and, unlike Dr. Squire's method, the application from one dressing to another is allowed to accumulate on the surface.

This ointment has been found of great service in relieving the suffering in some forms of pruritus, particularly that of the genitals and anus. Herpes preputialis has also given way to this remedy.

Were it not for fear of occupying too much space and becoming tedious, numerous cases could be cited of the above troubles which have rapidly yielded to the application of lead.

I have just learnt from my friend Staff-Surgeon E. Higgon Evans, R.N., that the form of ointment I have described has been used in that service with admirable results.—*Med. Times and Gazette*, April 29, 1876, p. 464.

77.—THE TREATMENT OF SCROFULIDES (LUPUS).

By Dr. HENRY G. PIFFARD, Professor of Dermatology, University of the City of New York.

[Lupus, in its usual varieties, is undoubtedly a scrofulous affection. Its treatment by the internal administration of arsenic is generally merely so much lost time.]

If we remember that the natural tendency of these affections is to extend indefinitely, and almost invariably to leave cicatrices, the indications for treatment are perfectly plain; namely, to check the spread of the eruption, and to cure the existing lesion with the least possible scarring. The details of treatment will vary with the particular variety under notice, and will be considered in the order in which they have been given.

Treatment of the Erythematous and Corneous Scrofulides (Lupus Erythematous).—Kaposi recommends frequent frictions with the *spts. saponatus kalinus*, and states that he has thus caused the lesion to disappear with leaving the slightest trace. I have had the same happy result follow the use of glacial acetic acid applied bi- or tri-weekly. Both of these applications act superficially and without producing an eschar, and seem to effect their purpose by causing absorption. I have likewise had excellent results follow repeated and energetic frictions with the following:—℞ Saponis viridis, oz. viij. Glycerini. Olei cadini. aa. oz. ij. Ol. rosarum. q.s. M.

These preparations should be used in preference to stronger caustics, which, destroying tissue, would necessarily be followed by a cicatrix. The only objection to this treatment is the length of time required. In a few more fortunate instances a cure may result in a few weeks from twenty or thirty of the frictions, or applications of the acid, but commonly their use must be prolonged for several months. These means are admirably adapted to recent patches, and may result, as observed by Kaposi and myself, in perfect cure. If, however, the eruption has already existed for some time, and portions of it, especially the central and older parts, begin to exhibit atrophic changes, it will be vain to hope for recovery without cicatrix. Under these circumstances the centre of the lesion may be left to take care of itself, and attention be confined to the periphery, that is to the advancing border. If the preparations spoken of do not appear to act with sufficient vigour, the pure *sapo viridis* (*sapo viridis* or green soap, so much used in Germany, is composed of spermaceti, lb. ij. sol. of hydrate of potash (sp. gr. 1.33), lb. j.) may be substituted for them, applied daily with friction, or spread upon a piece of linen and left in contact with the parts for several hours. If the green soap cannot be readily obtained, a solution of caustic potassa, 25 per cent., to 50 per cent. may be used instead, applied once a week or at more distant intervals according to the effect produced. The acetic acid and sol. of potash are usually applied with a camel's-hair brush or a glass rod. Both of these methods, however, are objectionable, as they are apt to deliver an excess of the fluid, which runs over and injures healthy parts. A much better way is to employ a small hard-rubber probe around the end of which a little cotton-wool has been wrapped. Stronger caustics than these mentioned are rarely required, unless speedy destruction of the lesion is necessary; in which case *potassa cum calce* or chloride of zinc may be employed. When the lesion is small and its early extinct desired, I prefer removing it (*ægroto volente*) by excision.

Dubina, and more recently Volkmann, prefer to remove the infiltration by means of hundreds of fine punctiform scarifications. These are made with a small sharp-pointed knife to the depth of 2 millimètres. The punctured spots should be covered with lint well pressed on, to stop the bleeding, and left attached until it falls spontaneously. The operation should be repeated in from two to four weeks, from three to eight operations being usually sufficient. Volkmann says that no scarring results from the treatment. I have had no personal experience with this method in the erythematous scrofulide, but have used a modification of it in the next variety.

Treatment of the Pustular Scrofulide.—The local treatment of

this variety is quite simple. As scarring is inevitable, we may attack the lesion with some vigour. If it be in an early stage, without much ulceration, I have had good results from linear scarifications, followed by green soap. When, however, there was considerable ulceration, it was formerly my custom to make a few superficial cauterisations with the liq. hydrarg. nitratis, followed by the application of a mercurial ointment (usually the proto-iodide, ten grains to the ounce). Neligan and Weisse have used with advantage the acetate of zinc. In this way the ulceration may sometimes be coaxed to heal, but the process is tedious, and the resulting cicatrix often unstable. Latterly I am inclined to prefer more rapid and energetic measures, such as thorough cauterisation with the chloride of zinc. The chloride exposed to the air until it has sufficiently deliquesced is applied to the ulcer in a thin layer by means of a platinum spatula. It is then covered with a piece of lint cut to fit the part, and the whole left to itself until the eschar falls, which may not occur for a week or ten days. Upon removal of the crust healthy granulations will usually be apparent (and sometimes complete cicatrization), but if not the cauterisation must be repeated. In case the floor of the original is covered with large, uneven, and papillomatous granulations (*verrucous form*), it is best to remove them by thorough scraping with the "sharp spoon," an instrument specially recommended in this connection by Volkmann. It is also well to give the edges a good scraping with the spoon. The morbid tissue yields more readily than the healthy, and a considerable portion of the disease may be thus removed mechanically. When a somewhat level surface has been obtained, apply the zinc in the manner described, and, after the fall of the crust, repeat the operation if necessary.

Treatment of the Tubercular Scrofulide without Ulceration, or with Superficial Ulceration.—If ulceration has not yet occurred, we have several methods of treatment from which to choose. The indications are to check the spread of the lesion, and to do so with the least scarring. Excision may be practised, and is to be preferred if the lesion is limited and there is sufficient extensibility of the surrounding skin to permit of coaptation of the edges of the wound, so that we may hope for a linear cicatrix. If the lesion is small and quite superficial, it may be readily removed with the skin-grafting scissors. Excision to be effectual must be thorough, both as to depth and extent. As the morbid deposit in this variety rarely extends beneath the subcutaneous tissue, and usually ceases at the lower part of the corium, no difficulty will be experienced in this direction; but the infiltration is apt to extend laterally for some distance beyond the points at which the disease is evident, and conse-

quently the incisions should be made at least two or three millimètres beyond the apparent limit of the lesion; otherwise a speedy return of the trouble may be anticipated. If the situation of the disease or the condition of the parts renders excision undesirable, the tubercles may be destroyed with nitrate of silver. This method is largely practised in Vienna, and consists in boring into the diseased tissue with a sharpened point of fused nitrate. The points of silver prepared by Squibb, to which he adds a little chloride, to make them harder, are admirably adapted to this purpose. Many of the tubercles, or rather papules, are quite small, and boring into them with the ordinary caustic points inflicts unnecessary pain and injury upon the adjacent healthy tissues. To obviate this, I have for several years used a probe made of irido-platinum. The point of the needle is dipped in fused nitrate (Squibb's is better for this purpose than the pure), a thin layer of which immediately congeals around it. As soon as it is cold, a second or a third dipping will increase the deposit of silver sufficiently. With these needles quite small punctures may be made, and are less painful than those made with the ordinary silver stick. If the irido-platinum needles are not at hand, ordinary straight surgeon's needles may be used, but are not so convenient and are soon destroyed. Actual or galvano-cautery may be employed, but possess no advantages over simpler measures, except the opportunity they afford for display.

Lastly, the disease may be destroyed by means of arsenical paste. Hebra speaks of this method so enthusiastically that I shall give his description in full. The formula employed is:—
℞. Acidi arseniosi, gr. xx.; cinnabaris, dr. j.; ung. rosæ, oz. j. M.
The paste should be thinly spread upon linen, which is cut in narrow strips and accurately applied to the affected part. It is then covered with lint and held firmly in place with adhesive plaster. It is left in position for twenty-four hours, when a fresh application is made without previous washing. At the end of another twenty-four hours it is again applied. During the first day the parts to which the application has been made present very little alteration and are not very painful. At the end of the second day of treatment, however, the pain increases, and when the plaster is removed the tubercles are found to have a greyish colour and macerated aspect. After the third application the pain becomes still more severe, and the integument surrounding the lupous patch becomes the seat of considerable oedema. On removing the plaster at the end of the third day, all the tubercles, both large and small, are found reduced to the condition of a brownish-black eschar, and covered with a thin pus. The eschars correspond to each tubercle, and are sharply defined by the intervening healthy skin. The pain

ceases quickly and completely, and the oedema disappears in two or three days. The great advantage of this arsenical paste consists in its not injuring or even excoriating the healthy skin, while the morbid tissue is safely and thoroughly destroyed.

The little sloughs, which are as numerous as the pre-existing tubercles, are loosened by suppuration, and removed in five or six days, leaving small holes separated from each other by islets of healthy skin. Cicatrisation is rapid, and the resulting scar not very noticeable.

Sometimes two applications are sufficient; or again, if the tubercles are large and covered with thick epidermis, two or more courses may be necessary. Arsenical poisoning has not occurred in any of the hundreds of cases treated by Hebra in this manner.

If the tubercles have already *ulcerated*, they may sometimes be coaxed to heal, as already stated, by stimulating applications of acetate of zinc and the like; but it is better to excise, subject to the restrictions above noticed. If incision is contra-indicated, the ulcer and its margin may be well scraped with the sharp spoon, and then cauterised with the chloride of zinc, or a mixture of equal parts of caustic lime and caustic potash made into a paste with absolute alcohol. The fall of the slough will usually reveal a healthy ulcer with a tendency to heal. I much prefer the caustics to the simply stimulating applications, as the latter have in my hands more frequently produced an effect opposite to the one desired.

In the *hypertrophic* form, excision when convenient, or destruction by the actual or galvano-cautery or by electrolytic needles, seems to be the most reliable method; but never having had this variety under treatment, I cannot speak from personal experience.

Treatment of the Tubercular Scrofulide with deep Ulceration.—This form demands the most prompt and energetic treatment. In the previous varieties it was at most a question of disfigurement, but in this it is a question of life and death, for if the destructive processes are not checked, a fatal result is by no means rare. In this condition there is but one method to be relied upon, and this is excision followed by cauterisation. In recent cases this presents no difficulty; but when the disease has already advanced to a considerable extent, and the bones have commenced to suffer, the indications are not so easy of fulfilment. These cases were formerly regarded as incurable, and active measures were not recommended. Mr. Moore, however, in his admirable monograph upon *rodent cancer* as he terms the affection, has demonstrated the possibility of curing cases characterised by a very extensive destruction of deep tissues, cases which would usually be regarded as hopeless. His plan

consists in removing the diseased parts as thoroughly as possible with the knife, and afterwards applying a layer of deliquesced chloride of zinc. The wound is then packed with cotton-wool, which is left in position till thrown off by suppuration and the formation of healthy granulations. Plastic operations may then be employed to remedy the deformity.

Treatment of the Phlegmonous Scrofulide.—In the early stages and before suppuration has occurred, the nodules may sometimes be caused to disappear by the employment of frequent frictions with the ungt. iodi or ungt. potassi iodidi; but if they have already softened and contain pus, this should be removed by a fine puncture and suction with a hypodermic syringe, with the subsequent instillation of a drop or two of iodine. This is much better than the free incision recommended by some, and is also better than leaving the lesion to take its own course, as in the latter case considerable scarring is inevitable. If the abscess has already opened and an ulcer formed, it should be freely stimulated and perhaps cauterised with some of the agents already mentioned.

Lupus in its several varieties is far from being the intractable disease it was formerly considered, and, when properly managed, is in the majority of cases perfectly curable. It is only a question of time, patience, and the judicious selection of remedial agents adapted to the particular case under consideration.—*Practitioner*, Dec. 1875, p. 415.

78.—THE TREATMENT OF PSORIASIS BY INDIA-RUBBER UNDERCLOTHING.

By Dr. BALMANNO SQUIRE, Surgeon to the British Hospital for Diseases of the Skin.

[Tar administered internally is no assistance to outward tar in the treatment of psoriasis. This was shown by Mr. Squire in a paper previously published. In the present paper he relates two cases treated by an agent of a different sort.]

Case 1.—A woman, aged 30, had been affected with chronic psoriasis in a moderate degree for 20 years, and, in addition to this, had for the past five weeks been the subject of an acute general attack of guttate psoriasis, which freely occupied every part of the skin, with the exception only of the fingers and the palms of the hand, and the toes and soles of the feet, but most copiously the front and inner surfaces of the thighs, the lower half of the abdomen, the loins, and the front of the legs and the back of the forearms. On February 1st, 1876, she commenced wearing a complete suit of vulcanised India-rubber underclothing, together with an oilskin cap, washing herself daily all over with warm soap and water. On February 8th,

the scaliness, the thickening, and redness of the skin at the spots had quite gone, and the eruption consisted merely of faded yellow stains, with the exception of a few spots on the legs. On February 12th, the stains had become much fainter, those on the forearms requiring to be looked at "across the light," in order to make them out.

Case 2.—A girl, aged 18, had been affected with chronic psoriasis for twelve months. The eruption was scattered in patches of varying size over the trunk and limbs (the larger patches occupying the front and outer surface of the lower part of the thighs and the front of the upper part of the legs and the back surfaces of the forearms and lower part of the arms), and on the scalp. On February 1st, 1876, she was clothed like Case 1, and washed herself daily in the same way. On February 8th, half the patches had disappeared, leaving only faint yellow stains. All the others had in part disappeared, some (for example, the two largest patches situate on the knees) having become only broken rings, which, a week before, had been large, filled in, figurate patches. On February 12th, quite three-fourths of the whole number of patches had disappeared, and the vestiges of the remaining fourth had become still scantier.

Remarks.—The device is not my own, but that of Dr. Colson of Beauvais. Experiments of this kind have since been pursued by Dr. McCall Anderson of Glasgow, Drs. Hardy, Lailier, and Besnier of the Hôpital St. Louis, Paris, and Dr. Hebra of the General Hospital, Vienna. No distress is produced by this kind of underclothing, nor, indeed, inconvenience of any kind, save only that, if the wearer exert himself or herself at all actively, a moderately profuse perspiration is the result.—*Brit. Medical Journal*, Feb. 19, 1876, p. 217.

79.—ON A MORE EFFECTUAL METHOD OF APPLYING IODINE TO THE INTERIOR OF CERTAIN CYSTS.

By FURNEAUX JORDAN, Esq., Surgeon to the Queen's Hospital, Birmingham.

I have found that there are in practice two classes of scrotal hydroceles, in which the ordinary methods of treatment are either difficult to use or uncertain in their result. In boys and men there are occasionally encysted hydroceles of the testis, or the cord, which continue to increase in size, or in which treatment is urgently requested. In such cases, except in early infancy, acupuncture or the use of a fine trocar often fails to cure. The walls of the cysts are usually thin, and collapse so much when their contents are withdrawn that the injection of a fluid is uncertain. The end of the canula may be outside the

cyst, and the iodine solution be consequently injected into the connective tissue at its exterior. In such cases the following is a reliable method of treatment:—The cyst being well isolated, made tense, and brought near the surface, I pass through its centre a stout needle, armed with silk, and leave the threads hanging. The fluid quickly oozes away, especially if a little traction be made on the threads. I then, at one opening, wet the threads with iodine liniment (liniment because the quantity required is so limited) and draw the threads so as to leave moistened portions within the cyst. A little gentle friction will help to spread the iodine thoroughly over the lining membrane of the cavity. An hour later freshly moistened portions may again be drawn through if the cyst be large, or if other methods of treatment have failed. On the other hand, in a very small cyst a single thread, moistened and kept in one hour, will suffice.

Another class of cases are those of simple vaginal hydrocele, in which the injection of iodine and other ordinary methods of treatment are unsuccessful. An interesting case will best convey what I wish to say. A young man had a moderate-sized hydrocele. Trocar puncture, and acupuncture repeated a few times failed, and consequently iodine tincture (undiluted) was injected. In a few weeks the collection had reached its old size. A silver-wire seton was then put in; while in, the cyst remained empty, but its removal was followed by reappearance of the fluid. I then, at three o'clock, passed through the cavity a double silk thread at two spots. In a few minutes, when all the fluid had oozed out, I drew the threads, moistened with iodine liniment, into the serous cavity. I directed him to repeat the process in an hour. He was so anxious to get well—he was shortly to be married—that he moistened the threads four times in six hours. At midnight the effects had become so sharp that he was glad to remove the threads as he had been directed. He remained at home one day only, and was shortly and permanently well.

I venture to believe that no kind of hydrocele will resist this method of applying iodine, and consequently that the setting up of suppuration, even as a last resort, can rarely be necessary.—*Lancet*, Jan. 29, 1876, p. 170.

SYPHILITIC DISEASES.

80.—ON SYPHILIS.

By Dr. v. SIGMUND, Professor of Dermatology and Syphilis in the University of Vienna.

In this paper I have confined myself to the results of the clinical observation of facts during a tolerably long career

spent in constant attendance on the Vienna General Hospital, and have not attempted to enter into histological questions.

Syphilis is a disease completely distinct and separate from all forms of gonorrhœa; on this point all observers are agreed. But there is a disease which is most frequently, and indeed almost exclusively, propagated by sexual intercourse, which is characterised by the appearance of an ulcer, usually, though not always, seated on the surface or mucous membrane, especially of the generative organs, or their immediate vicinity, and which, like gonorrhœa, is a *purely local* affection. In many cases, coincidently with the formation of pus in the ulcer, and sometimes with its confinement and burrowing in the connective tissue and skin, *inflammation arises in the lymphatic vessels and glands*, but in all instances it does so only in the neighbourhood of the ulcer, and always *in direct anatomical relation and connection with the seat of the ulcer*—never without this, never isolated or at a distance from it.

This inflammation runs its course *without* in any instance causing general disease of the organism. Thousands of cases have fallen under my notice in which I have been able to satisfy myself on this point by observations continued for years. Moreover, I have seen cases of repeated infection, and in one of our ablest soldiers, who died a hale old man at the age of 83, without offering *a trace* of constitutional syphilis, the infection was repeated as many as twenty-four times. Such ulcers constitute, it is well known, the subject of dispute between the so-called dualists and unicists. Nothing that has been adduced on either side is capable of absolutely deciding this question: clinicists must however maintain the dualism of the disease, until better grounds than those at present advanced for unicism have been discovered. The so-called venereal ulcers furnish inoculable pus which even in syphilitic patients always reproduces syphilis in all stages, phases, and forms, and these ulcers make their appearance in individuals who have never suffered from syphilis, just as upon those who have long suffered from it and now appear to be healthy. Undoubtedly under the general term ulcers, different forms of disease are included, as diphtheritic ulcers, contagious venereal ulcers, follicular ulcers, &c.; nor can there be any doubt that loss of substance of the skin, in all points similar to the so-called soft chancres, can be produced by the inoculation of simple pus. Precise histological as well as chemical and clinical researches will alone establish the fact of what I have just stated. All the attempts of the unicists to view these ulcers as different grades of a true venereal sore depending on the greater or less intensity of the syphilitic virus, are untenable on an unbiassed examination. The virus of

syphilis has further the peculiarity that it is not simply communicated by contagion, but that it infects, *non contactu sed infectione sanguinis et nutritionis totius organismi agit*. And a poisoning of the blood and consecutively of the nutrition of the body at large, syphilis is, and remains, from its first germination at the point of contagion to its passage into the lymphatics, and from thence into the blood vascular system, until finally it passes from the defined forms which it assumes in various systems and organs, into the ultimate syphilis-cachexia. That the germ of syphilis does not produce in every one who may be affected, *all* the well-known series of symptoms indicative of such morbid phenomena is a remarkable fact; but this is explicable if the individual attacks are studied carefully and for a sufficient length of time. For example, I have directed the attention of my pupils for years to the comparatively mild course of syphilis in the menstruating woman, but I have demonstrated many other causes of such a mild course, which all point to the fact that its course is simpler or milder when we strive to preserve the several physiological junctions as far as possible in a healthy state, to remove all causes of disturbance, and moderately to increase the secretions and excretions, whilst as far as possible we prescribe a complete system of hygiene and dietetics, along with mercury or the iodides. That the *former* in the earlier stage (up to the papular eruption) is only rarely and exceptionally applied as a general remedy has often enough been seen in my clinical practice.

Now there is still another point, viz., the inoculability of syphilis. If we were going to speak of experiments on this point, the mere methods of performing them would require special attention and criticism, and observations of their results and conclusions founded upon them would do so still more. But we will pass them over and consider accidental inoculations, in regard to which the following facts have occurred to me. So-called primary and secondary syphilis is inoculable: upon this point all are agreed. The gumma too, and, I repeat, the substance of the gumma as well as the blood taken from the gumma, is also inoculable. This is associated with the fact that gummous mothers bear children which do not at first appear syphilitic; but often, no doubt, distinct though perhaps not glaringly palpable evidences of the disease are overlooked during the first years of life. These children subsequently present unquestionable gummatous forms of syphilis. Let not the dogmatists be too hastily misled by the still more hasty critics. I venture to give this warning to those who believe the question to be decided as to whether father or mother give congenital syphilis. I have never succeeded in recognising true syphilis in a child in which the uterus that

bore it was not also syphilitic. But unfortunately I know many, many cases in which the uterus, on *scrupulous examination and protracted observation*, was found to be diseased in women considered to be healthy so far as syphilis was concerned, and where the syphilis in the child had been attributed to the semen of the father.

When trouble and time were taken to investigate carefully and for sufficiently long periods all the organs and systems of the body, it was found that besides general syphilis of the lymphatic glands, very frequently obstinate necrobiosis of the epithelium and of the corium existed in the cervix uteri, in the tonsils, the pharynx, &c., as well as extremely slight annular syphilides of the skin, very delicately marked psoriasis, &c. Time forbids me to enter further in this paper on the subject of the local and general disturbances of the blood-forming organs, and of the nutrition of the body, which occur in such persons.

But the whole chapter of this so-called latent syphilis (*latet illum qui non noscit*) is of the utmost interest to every practitioner who does not consider his whole duty to consist in paying attention merely to one form of the disease or to a few short moments in the life of the individual.

When more than twenty-five years ago (in complete accordance with my honoured guide Virchow) I declared syphilis to be a disease essentially affecting the whole lymphatic system, I had carefully studied the question of the extension of syphilis germs in not a few individuals. In perfect accordance with Virchow I have always regarded syphilis in all its stages as a disease which has its origin in new cell-growth, which cell-growth is incapable of becoming vitalised and soon passes into organic misformations and degeneration of various kinds. On this account I have also called it "trophic disease." It is for this reason also that in my clinical work I constantly inspect and examine the *whole* body. Nothing is more prejudicial to a thorough pathology of syphilis than to regard it as a skin disease, because many things are more readily observed on the skin than elsewhere *in primo introitu*. *Lenitius novit qui penitius pervestigavit*. Even at that time I directed the attention of the profession to this characteristic mode of extension *as the most constant type of the disease*. Look at the most recent histological researches, and if you inquire without prejudice you will see that from the simplest cell-infiltration to the gumma, according to the mode of the origin and extension it will always be found that first the lymphatic vessels, then the most delicate capillaries of the blood-vascular system, and ultimately the lymph and blood themselves, form the carriers of disease. This fact affords as complete an explanation even of the gummatous

disease of the vessels in syphilis of the brain as it does of the origin of these minute gummatous nodules and nodes in the male organs of generation, which often become very hard in chronic cases of syphilis, and are then regarded as cases of reinfection and of new syphilitic infiltration of a foreign nature.

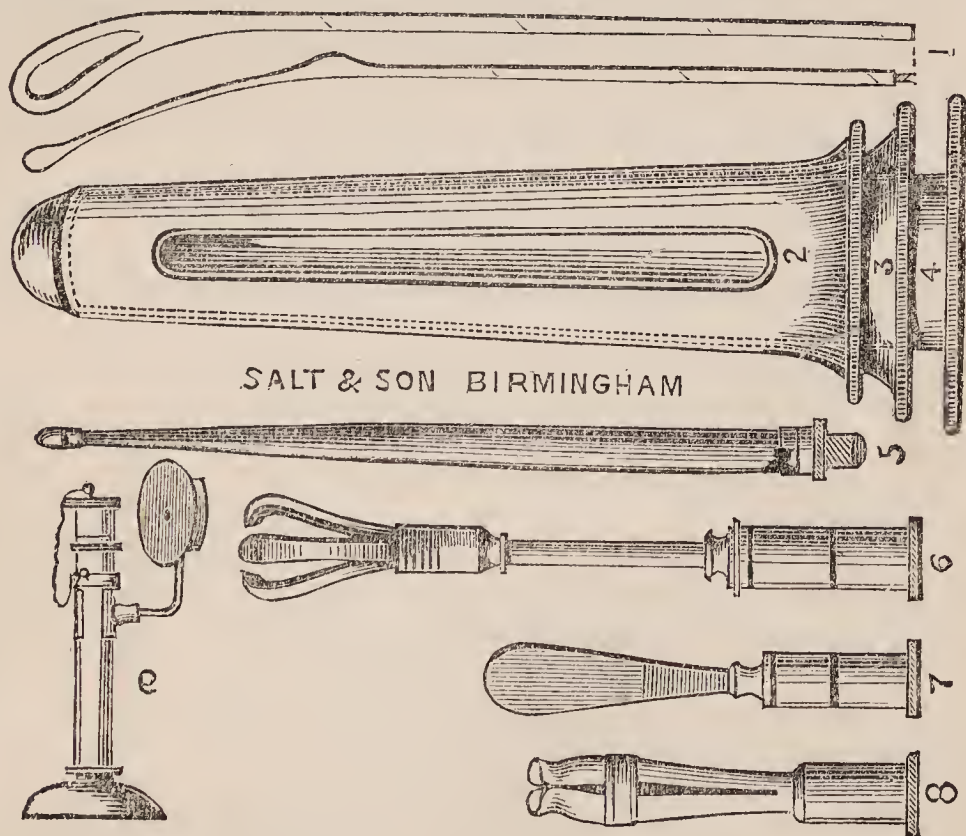
We have recently seen hard nodules of this description in the labia majora of a woman, who also presented similar gummata of cartilaginous hardness upon the body.

Now it is well to discuss the subject of syphilis, since science and humanity can only benefit from such discussion. Let all personal motives be thrown aside; one thing comes out clearly from these discussions. Public instruction has still a very important duty to fulfil in regard to medical science. Special classes for the clinical study of syphilis ought to be formed everywhere, and practitioners should everywhere be instructed in them in such a manner that they should be made to diagnose, even if they are not always made to treat, all forms of the disease. I need hardly observe that thereby the most effectual barrier would be erected against the ignorance, arrogance, and charlatanry that prevail in this much dishonoured region of scientific and social hygiene.—*Practitioner*, March 1876, p. 191.

MIDWIFERY, AND THE DISEASES OF WOMEN AND CHILDREN.

81.—NEW SPECULUM VAGINÆ.

We have examined the new speculum of Messrs. Salt and Son, of Birmingham, and find it a compact and convenient instrument. The novelty and utility of this invention consist in the economising of space, by arranging for the supply of a number of instruments in constant request by the obstetrician, without any increase in the bulk of the speculum. All the instruments illustrated below (except the lamp) are contained within the introductory plug, hereafter described.



The speculum consists of two tapered metal tubes (Nos. 2 and 3), highly polished for reflection, fitting the one within the other, and capable of being used independently; thus forming two specula of consecutive sizes. Both have a lateral opening, as seen in the engraving; the inner one may, by rotation, close the lateral opening wholly or in part, so that any desired sur-

face of the vaginal walls can be exposed for inspection or operation. The plug (No. 4) fits accurately the inner speculum, and has its end "coned," to facilitate introduction. It is closed at its larger extremity by a movable metal cap, which serves as a reflector for a candle-lamp. The complete speculum is contained in a neat leather case, and occupies, with the following instruments, no more space than an ordinary Ferguson's speculum:—1. Simpson's sound, united by a screw or hinge-joint to a fenestrated elevator (No. 1). 2. Port-caustique, lancet, and sponge-holder (Nos. 6, 7, and 8), each fitting into a porcupine-quill handle of suitable length (No. 5). 3. Candle-lamp and reflector (No. 9). 4. Sponge and laminaria tents.—*Lancet*, Feb. 26, 1876.

82.—ON THE INDUCTION OF PREMATURE LABOUR.

By Dr. CLEMENT GODSON, Assistant Physician-Accoucheur to St. Bartholomew's Hospital.

[After considering the reasons for having recourse to the induction of premature labour, the author of this paper passes on to the consideration of the various plans which have been proposed for its performance.]

First, let me enumerate them in succession, and then discuss them separately—

1. Evacuation of the liquor amnii by puncturing the membranes.
2. The administration of certain drugs, particularly ergot of rye.
3. The injection of water into the vagina.
4. The injection of water within the uterus.
5. The injection of atmospheric air or carbonic acid within the uterus.
6. Galvanism.
7. Irritation of the mammæ, by means of cupping-glasses.
8. Separation of the membranes from the uterine wall, as far as is practicable, with the finger.
9. Insertion of a long gum-elastic catheter between the membranes and the wall of the uterus.
10. Dilatation of the vagina by means of air-bags.
11. Dilatation of the os uteri by air-bags.
12. Dilatation of the os uteri by means of sponge tents.

1. The old method of puncturing the membranes is still in favour with a great many. I must say that I am strongly opposed to it as a rule. I perfectly agree with what Baudelocque says when arguing against induction of labour. It is the *plan*, however, which he condemns. Probably, had that been known to him of dilatation by sponge tents, his opinion would have been very different.

He says, "If we break the membranes before the orifice of the uterus be sufficiently open for the passage of the child, and the action of that viscus strong enough to expel it, the pains will go off in the same manner for a time, and the labour afterwards will be very long and very fatiguing; the child, deprived of the waters which protected it from the action of the uterus, being then immediately pressed by that organ, will be a victim to its action before things be favourably disposed to its exit, and the fruit of so much labour and anxiety will be lost." What reasoning can be more sensible than this?

And yet, in the *British Medical Journal*, during the last few months, a Medical Practitioner in Cornwall has been attempting, in a series of letters, to run down the sponge method, most ably advocated in a paper by Dr. Swayne, of Clifton, and claim superiority for the plan of evacuating the liquor amnii, his whole experience of which is confined to a single case occurring in his practice, which happened to result satisfactorily, just as one hears of the waters breaking in the street, and the child being almost born before the patient could reach her house. But, I ask, is it a matter of congratulation, as a rule, when one finds that the membranes have ruptured before the os uteri has hardly commenced to dilate? Does not experience tell us that the consequence, generally, is a long, tedious labour?

2. The system of administering ergot of rye as a means of bringing on labour prematurely has two great objections. First, it is excessively uncertain in its action; my experience is, that it acts much better in increasing the uterine contractions which have already commenced, while most frequently it fails entirely to start them, even when enormous doses have been given. And it is believed that the drug, taken in large quantity, exerts a direful influence upon the foetus in utero. I think, therefore, that it should never be employed as the *sole* means of inducing labour.

3. The injection of water into the vagina. This has been employed at a temperature of from 95° to 100° F. for half an hour at a time, the douche directed against the os uteri every four or five hours. The late Dr. Tyler Smith recommends hot and cold water alternately. Dr. Barnes says the vaginal douche takes one, two, or more days, and is liable to cause congestion of the lower segment of the uterus. For my own part, the inconveniences of the application, and the uncertainty of its action, would preclude my ever having recourse to it.

4. The injection of warm water within the uterus, which is known as Kiwisch's plan, was introduced by Schweighauser in 1825. Cohein, of Hamburg, in 1846, used it with creosote added. Numerous successful cases have been published; but there is one objection to it, which should prevent its ever

being again attempted. It places the life of the mother in jeopardy. Lazzati has collected 36 cases in which it was employed, and of these, 12 mothers died. Surely no stronger evidence against it need be adduced. Considering that there should be almost perfect immunity to the life of the mother after other methods, no more need be said to utterly condemn the practice. But Tyler Smith, in his Manual, strongly recommends it; and as this is popular among students, and very deservedly so, for the clear way in which the mechanism of labour is described, care must be taken not to accept his views upon this special subject before us. It has been demonstrated that the water may pass through the fallopian tubes into the peritoneal cavity; air may be injected into the uterine sinuses; the placenta may become detached; the uterus may be so distended as to become ruptured; or the patient may die from simple shock.

5. The same objections apply to the injection of carbonic acid or atmospheric air, though successful cases of the former have been published by Scanzoni, and of the latter by Sir James Simpson; but deaths from both have been recorded.

6. Galvanism was proposed by Herder in 1803, in order to excite uterine action; but it was not applied to the purpose which is before us till 1844, when Dr. Radford, of Manchester, commenced to employ it, and did so successfully in four cases. Sir James Simpson and Dr. Barnes have also, formerly, advocated it. Having so much more convenient a method at our disposal, I should never employ it, for it must be most objectionable to the patient, and tedious to the medical attendant.

7. Scanzoni proposed, and tried, the effect of a sucking-pump to the mammæ for the space of two hours. He was successful in two cases: one required seven applications, the other but three. It seems to me a very uncertain and disagreeable method, and one likely to interfere with lactation subsequently, from the irritation to the breasts caused by it.

8. Separation of the membranes from the uterine wall, as far as can be reached with the finger, is a very simple method, and one which in some cases may probably be sufficient; but certainly in the majority of instances further means would have to be adopted. There can, however, be no objection to trying it.

9. Inserting a long gum-elastic catheter between the membranes and the wall of the uterus, and leaving it there, coiling up the end in the vagina, has been found very effectual, but certain objections have been raised to it. It is said that the placenta *may* be partially separated by it, and the membranes *may* be accidentally punctured while pressing it. These should be improbable consequences, and I believe it to be one of the

best methods which we have; for, should it fail, it may be followed up by the insertion of sponge tents.

10. In America, the term "colpeuryasis" is used to signify dilatation of the vagina, and a vulcanised gum-elastic bag called Braunn's 'colpeurynter has been employed to dilate the vagina, with the view of setting up uterine action. While stretching the vagina, it is supposed to act also upon the os uteri, and Dr. Meigs says it is found that about four hours suffice to dilate the os uteri sufficiently to provoke good labour pains. I do not know that it has ever been carried into practice in this country.

11. Dilatation of the os uteri by elastic bags was proposed by Dr. Keiller, and has been brought prominently forward of late years by Dr. Barnes, who has introduced an improved form of bag for the purpose.

Speaking of the different methods for provoking the uterus into action, Dr. Barnes says, "Now, active labour may come to pass in twelve or twenty-four hours, or in two, three, four days, or even later. There is no certainty about it. When labour comes, the child is expelled with little warning, almost suddenly, and before the medical attendant can be fetched. And it has to run the gauntlet of all those perils which especially surround premature labour unaided. It is just as feasible to make an appointment at any distance from home, to carry out at one sitting the induction of labour, as it is to cut for the stone." Dr. Barnes puts in an elastic bougie over-night, which, he says, gives rise to uterine action; next day he introduces his bags. Uterine action has been set up by the bougie, in other words, labour has commenced; 'why then interfere with a process which, started artificially, may be completed gradually by the efforts of nature? If this argument of carrying out the process of labour "at one sitting, as one would cut for stone," applies in premature labour, why should it not in labour at full term? If not likely to be followed by evil consequences, it would be a great boon to the medical attendant. To be kept about by a patient whom one has found in the first stage of labour is extremely trying. How much time and annoyance would be saved if we could proceed at once to "carry out at the one sitting" the delivery! And I cannot see why it should apply in one case more than in the other. Barnes' bags are of the utmost value when it is desirable to complete the labour in as short a time as possible—for instance, in puerperal convulsions; but in induction of premature labour, for causes not requiring hurry, to my mind their employment partakes too much of an operation, when there is no necessity for such discomfort to the patient.

Lastly, we have to consider the method of dilating the os uteri by means of sponge tents. It was proposed and put into practice with great success by Brüninghausen and Kluge; Velpeau strongly advocated it; and finally, Sir James Simpson, who had tried a variety of means, adopted this in preference to any other. I am convinced, with him, that it is by far the most preferable. In 1872 I wrote a paper in the *Lancet* in favour of it, describing an instrument which I had designed for facilitating the introduction of the tent. Dr. Bedford, of New York, says, "It may be found extremely difficult, in consequence either of resistance or malposition of the os, to introduce the sponge, and the abortive attempts made to accomplish the object may induce more or less irritation of the parts." I found also this difficulty, to overcome which I had the instrument made.

Quoting from my paper in the *Lancet*—

"The means I advocate operate by surely and safely coaxing the uterus into an action which only differs from natural labour in being artificially initiated, and which is maintained and completed, under all the conditions of labour spontaneously occurring at a corresponding stage of pregnancy. Each of the methods in general use is, according to my experience, more or less formidable, in virtue of the amount and the kind of the manipulation which it involves. Most of them are practised in such a manner as to force on too hurriedly the uterine contractions; and that which consists in the evacuation of the liquor amnii stands self-condemned, as depriving the womb, at the very outset, of the all-important dilator provided by nature.

"My mode of procedure consists in insinuating, night and morning, between the cervix uteri and the membranes, sponge tents of gradually increasing size; the first, and each succeeding one, being as large as the parts will admit. On removing each tent, and before replacing it by another, a warm douche, containing Condyl's fluid, is administered. I have found the use of one, two, and three tents to be sufficient, and have never had occasion to employ more than four.

"The instrument by means of which the tent is placed in



position is made for me by Messrs. Arnold, of West Smithfield. It is shown in the accompanying illustration, and will be found fully described in the *Lancet* of April 22, 1871.

“It entirely obviates the use of the speculum, and being provided with what is equivalent to a universal joint, it enables the tent to be pushed, without extraneous guidance, between the cervix and the membranes, taking of itself the readiest path presented to it. For the same reason the membranes run no risk of puncture. The tents themselves are short, rounded at the extremity, and perforated, to facilitate adaptation to the instrument.

“The apparatus, and the mode of its application, are so simple, and so free from inconvenience and danger, that its use causes in practice little or no anxiety on the part of the patient; and until labour sets in, she moves about without pain or inconvenience, regardless of the presence of the tent.”

The plan was introduced by me at the City of London Lying-in Hospital in 1869, when I held the appointment of Surgeon-Accoucheur to that institution, and it is still carried on there by my successor, Dr. Burchell.—*St. Bartholomew's Hospital Reports*, 1875, p. 33.

83.—THE SURGICAL TREATMENT OF PROLAPSUS VAGINÆ. By Dr. THOMAS SAVAGE, Surgeon to the Hospital for Women, Senior Surgeon to the Birmingham Lying-in-Charity.

[After some preliminary remarks Dr. Savage relates two cases.]

The first of the two cases is that of Mrs. E., a private patient, aged 47 years. She had had five children and one miscarriage, the last child four years ago. Has suffered from bearing-down for years, but of late has been much worse in this respect. She had worn a pessary, a globular one, given to her by a medical man, who told her her womb came down. When first seen by me, the anterior vaginal wall was down, and there were some vascular growths round the meatus. These latter being very painful were excised, and chloride of zinc applied. Nitrate of silver was repeatedly used to the vaginal mucous membrane with considerable relief, it being very red and brawny. A further operation was recommended; no pessary could be borne; and after some months of no treatment at all, she came again, saying she would undergo any operation, as her distress continued to increase. She had been a widow for some years, and having to gain her own livelihood, had been compelled to stand very much. The vagina was very lax; there was a profuse yellowish white discharge, and the anterior vaginal wall was quite prolapsed, the projection of it below the meatus being very prominent. The

uterus was lower down than normal. Under chloroform I excised a piece of the mucous membrane of the anterior vaginal wall, of the shape of a lozenge, each side being about one inch long; stitches were then passed through the centres of opposite sides, brought together and tied. This caused considerable contraction, and would give to the cicatrix a perfectly stellate character. A parallelogram of mucous membrane was then excised from each side of the vaginal wall, the upper parts being on a level with the meatus, and about one inch from the muco-cutaneous edge. The incisions were about an inch and a quarter in length and three-quarters in width. The opposite sides were brought together. I then pared the posterior commissure of the vulva to the extent of about three-quarters of an inch in width, but not so long as for an ordinary case of ruptured perineum, viz., about two inches in extent. And two deep silver wires were inserted to bring the sides in apposition. Quills were not used, as it was feared they might more likely cause sloughing, which was especially to be dreaded in this instance, with three other wounds in the vagina to be healed at the same time. The ordinary after treatment was adopted, and healing of the wounds readily occurred, making the woman, as regards the comfort and enjoyment of life, quite a different person. She was able to stand for a lengthened period, and do her ordinary housework without any pain or even inconvenience. She was frequently seen for some months afterwards, and the relief continued, promising to be permanent.

The second case, Mrs. G., æt. 49, had had eight children and two miscarriages, the last child seven years ago. Suffers from bearing down, and has worn a support. On examination, when she strains, a cystocele is formed; the os is ulcerated; the vagina is very capacious, and a rectocele comes down. She wears a ring pessary for a time. She was afterwards admitted as an in-patient, and after a short rest in bed, the effect of which was to give her some relief, I had her anæsthetised by ether, and removed a lozenge-shaped piece of mucous membrane, one inch and a half each side in extent, from the anterior vaginal wall, one point being close to the cervix, and the other close to the meatus, the opposite sides being simply brought together by three catgut interrupted sutures; then a triangle of mucous membrane, one inch each side in extent, with the base downwards, from the posterior vaginal wall, three catgut sutures being used to bring the opposite sides together. The posterior vulvar commissure was then vivified, as in ordinary perinæorrhaphy, and brought together with silver wires. The catgut in the front wound appeared to become absorbed, that in the posterior wall could be felt when she left the hospital.

The relief in this case was just as marked as in the preceding.

The cases to which these or similar operations are available, all having in view the contraction of the cavity of the vagina and its outlet, appear to be women who have passed the child-bearing age, in whom the vagina is capacious and lax, with either complete or partial cystocele, rectocele, or uterine prolapse; which may have been induced suddenly, as by a violent effort; or, as is most general, gradually, as by increased weight of the uterus, acting from above, and forcing the organ to descend, first stretching the utero-sacral ligaments; or, by loss of support from below, as the loss of the padding of fat round the vagina; or, by a ruptured perineum. Of course, it is presumed the patient has been previously subjected to rest in bed, which has given only a temporary relief. In a few cases, I believe, rest in bed has a directly curative effect. Dr. Matthews Duncan has gone so far as to say that a ruptured perineum has very little effect, if any, in favouring the descent of the uterus. Madame Boivin speaks of the utero-sacral ligaments as the chief uterine supports, and this is confirmed by the experiments of Dr. Henry Savage, who found, on making traction on the uterus in a dead body, that these ligaments were the first to be put on the stretch. Most other authorities agree to this conclusion. The utero-sacral ligaments are described as the "two horns of a crescentic muscular emanation from the back part of the uterine cervix and platysma, with additional fibres from its vaginal attachments to each cornu." But, on the other hand, if the vaginal walls, when in normal relation and tone, are in apposition, and act as a column of support to the uterus, it would follow that to alter or destroy this column, as is done when the vagina is lax or the perineum gone, would give the uterus much more freedom to descend, and put greater tension upon the ligaments. Dr. Savage says in his third edition, just issued, "A vaginal orifice, preternaturally dilated, leads, sooner or later, to vaginal invagination, and with it necessarily uterine prolapse." And if the ligaments are stretched in addition to the loss of support from below, to restore the latter would give the ligaments an opportunity of regaining their lost tone, and so effect a real cure.

Some men think that no treatment of prolapse can be considered satisfactory on account of the complexity of the accident, and of the inability to apply the treatment to the origin of the disorder, saying that all aids, by mechanical or other appliances, are simply palliative; but, because we theoretically fail, surely we are not justified in losing sight of all the many means at our disposal, certain of them in certain cases approaching very closely to a radical cure.

The cases of uterine prolapse that would be deemed suitable then would be those which Bernutz describes as procidentia without "allongement," and, of course, would not include those where hypertrophy of either the supra or infra-vaginal portion of the cervix was affected, which would require to be treated, in the first instance, at all events, by a different surgical procedure, viz.; removal by amputation or otherwise, of the hypertrophied portion: And the reasonableness of operating in these cases is certainly affirmed if we observe the order of events in most cases. When such a woman is told to bear down, the first thing noticed is the prolapse of the bladder, on more forcibly bearing down the rectum usually next protrudes, though this sometimes does not occur on account of the areolar connections being so much looser in regard to the vagina than in the case of the bladder, and, on further effort the uterus is the organ which generally descends, the last of the three.

The treatment by narrowing the vaginal cavity was, I believe, first suggested by Dr. Marshall Hall, in 1831, though the subject is not mentioned in his book on the "Diseases of Females." He got the idea from Sir Henry Holland, who had introduced a pessary to support a completely prolapsed uterus for a young woman, which induced great inflammation; this was followed by such firm contraction of the vagina, that the uterus ever after remained in its proper position, and Dr. Hall relates at considerable length the details of his case, which was a complete success.

Then the late Mr. Baker Brown has done good service in the same direction, and in his book on "Diseases of Women" relates many instances of operations. It was from this latter that I was induced to try my first case; and he depicts in his work the shape and position of the pieces of mucous membrane to be removed. Dr. Henry Savage, thinking Mr. Brown's operation not sufficient, includes in his resection all the redundant vagina at its ano-vulvar margin in the first place; and in the second, the removal of a triangular portion of vaginal mucous membrane, the middle angle extending to some distance upwards, along the posterior wall of the vagina, securing with quilled sutures in the usual way. My own impression of this latter detail is that it is preferable to use separate sutures for the triangular piece in the posterior wall, as was done in my second case, where catgut was used and succeeded so well.

Marrion Sims suggests an operation by removing a V shaped piece from the anterior vaginal wall, extending with its points from close to the meatus to the base of the V close to the os; the opposite denuded arms are brought together, and a little

pouch is left in the centre, into which the os may slip and be supported. This he calls elytrorrhaphy. His principle consists in constricting the vagina superiorly, which is thus effected by removing part of the vagina; the plans of Baker Brown and others were to constrict the vagina along the floor. In the two cases related above, both principles were relied upon, and, I think, with the advantages that might be expected from both. —*Birmingham Medical Review*, April 1876, p. 110.

84.—ON THE COMPLETE EVACUATION OF THE UTERUS AFTER ABORTION.

By Professor ALEXANDER RUSSELL SIMPSON, President of the Edinburgh Obstetrical Society.

[What is the best way of securing a complete evacuation of the uterus in cases of abortion and miscarriage? It is often a serious thing to leave anything in utero in such cases, and the results may appear long afterwards.]

When we have to do with a case of abortion in which the stage of expectancy is clearly over, and the patency of the os internum, or the persistence of the pains, or the hemorrhage long continued or profusely flowing, call for active interference, there are two main indications to be fulfilled, viz., 1st, to restrain the hemorrhage, and, 2nd, to procure the perfect removal of the ovum.

I. *Restraint of Hemorrhage*.—In treating of the fulfilment of the former of these indications most of our manual writers are satisfactory enough, though even under this head there is some room for improvement. In a typical case we place little reliance on rest, cold, styptics, and so on, for we know that we can only effectually restrain the hemorrhage either (1st) by compelling the uterus to more energetic contractions, or (2nd) by blocking up the channel through which it escapes. In most cases we pursue both objects simultaneously.

1st. The uterus must be compelled to steady action by the administration of ergot, introduced in the form of a strong solution of ergotin in the subcutaneous cellular tissue—a mode of administration of the drug which is destined, I believe, to supersede those with which we have so long been familiar; for the subcutaneous injection of ergotin sets up the uterine contractions with such speed, such certainty, and such safety, that the hypodermic syringe will be found an indispensable part of the furnishing of every obstetric bag.

2nd. Pending the onset of effective pains, and with the view of at once hastening them and preventing the escape of the blood, we plug the genital canal. For the most part, and as if it were the best in all cases, authors recommended the use of

the vaginal tampon. Now, the plugging of the vagina, by whatever material, is sometimes the only expedient to which we can have recourse, and we may rely on it with much certainty for averting the immediate danger of excessive hemorrhage. But it seems to me that, where we have our choice, its application ought to be restricted to those cases where there is still some hope that, if we succeed in arresting the hemorrhage, we shall at the same time succeed in averting the impending abortion. Otherwise we have a means of controlling the bleeding more directly, at once less irksome to the patient, and less troublesome of application by the practitioner, in the cervical plug. For this purpose there is nothing so efficient as a good-sized sponge-tent, prepared, as Messrs. Duncan, Flockhart, and Co. now always prepare them, by being dipped in a disinfectant solution before being compressed by the whipcord. In my judgment, nothing can take the place of the sponge-tent for rapid, kindly, and complete dilatation of the cervical canal. And when we have introduced a sponge-tent through the cervix uteri in a case of unavoidable abortion, we may count upon a threefold effect. *First*, The bleeding is arrested immediately and effectively; for if there be any escape into the uterine cavity, it has simply the effect of swelling up more rapidly the sponge, which blocks up its canal of exit. *Second*, The uterus is stimulated to more rapid and more energetic action, as it always is when a foreign body is introduced within its cavity. *Third*, The cervical canal is being effectually expanded during the lodgement of the sponge within it; and where a sponge-tent of good size has been well planted, it secures, as no other tent that I know of does, the expansion of the sphincter of the os internum. When, by the adoption of these measures, we have fulfilled the first indication, we have at the same time prepared the way for the carrying out of the next, viz.,

II. *The Complete Evacuation of the Uterus.*—It is in describing the methods of securing this object that I find systematic writers particularly defective. For the most part, they tell us that when the uterus fails to expel its contents, these must be extracted with the finger or fingers passed through the cervix, and hooked round the ovum or placenta, or whatever fragment may still be *in utero*; and that, when it cannot be reached with the fingers, cautious use should be made of some forceps or other instrument. But then the methods by which the uterine cavity can be reached through the vagina are hardly more than hinted at, with a few exceptions, as by Cazeaux, who, in a single line, speaks of “strong pressure upon the hypogastrium to depress the womb;” and Schroeder, who, somewhat more fully, describes the bi-manual manipulation. If, *e.g.*, we open

the latest work, the deservedly popular "System of Midwifery" by Professor Leishman, we read—"The finger is to be cautiously passed round the protruding portion, and, if necessary, another finger may be introduced into the os. If we can thus succeed in getting a hold of the placenta upon which we can rely, it may be extracted entire; but a rude or unskilful mode of manipulation may entirely frustrate our efforts by leaving behind a portion of what we wish to extract whole. It is impossible to lay down rules for the skilful performance of this manœuvre, which can only be taught by experience; but we have no doubt that more reliance is to be placed upon the fingers than upon instruments, as a general rule." It is little wonder, if, a page further on, we find him speaking on the after-treatment (p. 434) of fragments of placenta giving rise again to hemorrhage; and all that is said of the treatment in that case is, that "the patient must not be permitted to rise until all trace of this has ceased!" Why, the abortion in that case is unfinished, and we have no right to expect a cessation of the hemorrhage until the delivery has been complete. It always seems to me that there are few obstetric manœuvres capable of clearer exposition than the manipulations that we employ for emptying the uterus in the last stage of an abortion. Let us first have a clear idea as to what is required to be done. We have to pass one or more fingers into the cavity of the uterus to explore the entire cavity, to separate from its walls any adherent portion of the ovum, and then to extract the separated mass. I say at once, and simply, that it is the fingers of the operator that are to do the intra-uterine work, for I am quite at one with those who deprecate the use of instruments, such as curettes, wire-loops, crotchets, and abortion forceps, for the *detachment* of retained ova or their fragments. When the adhesions have been all separated by the finger, it may sometimes facilitate the removal of the loosened body to seize it with such instruments, and in that case, a pair of long dressing forceps, or polypus forceps, suits as well as any special implement; but the detaching of the adherent portions of the placenta should in no case be intrusted to these, or attempted with them, seeing that it can always be effected by the use of the sense-guided finger.

But how are we, with the finger or fingers, to reach so high in the cavity of the uterus as completely to surround the ovum? To begin with, the patient should, as a rule, be anæsthetized. The manipulations necessary to secure a satisfactory result cause suffering, though not to a great degree, which we can always save the patient by bringing her under the influence of chloroform. And at the same time that her sense of pain is abolished, her voluntary muscles are completely relaxed, and it

becomes easy for the practitioner to press down the uterus through the abdominal parietes. Once and again I have found myself baffled in the effort to reach the fundus uteri in such a patient until I had chloroformed her; for, however willing the woman may be to further your efforts for her delivery, involuntarily she contracts the recti abdominis when you make pressure on the hypogastrium, or withdraws herself when you press the other hand against the perineum. The patient, then, having been anæsthetized, we may render the uterine cavity accessible to the exploring finger in one or other of two different ways.

1st. We can push down the fundus uteri from above. The patient may lie either supine or in the ordinary obstetric position on her left side, with the knees drawn up; most frequently the right hand will be used for internal manipulation, while the left is applied to the abdominal surface. It rarely suffices to pass one finger alone into the vagina. In most cases the index and middle fingers are passed into the vagina, and while the middle finger is folded in the fornix to steady the uterus there, the forefinger is passed through the cervix. Or the middle finger can sometimes be more satisfactorily employed for the intra-uterine digitation, or, better still, both fingers may be passed into the uterine cavity. In the last case it may become necessary to have the other two fingers carried into the canal of the vagina, the thumb alone remaining external to the vulva. It is usually only in patients who have miscarried at the fourth month, or beyond it, that the hand requires to enter so far for the separation of the placenta, and then the vaginal cavity is relaxed and roomy enough easily to permit of it. Whilst the fingers of the right hand are thus seeking their way up to the recesses of the uterus, the left hand applied above the brim of the pelvis, is pressing the uterus forcibly and steadily downwards into the pelvic cavity. In this way, in the great proportion of cases, we obtain perfect command of the uterine contents. The fingers of the two hands recognise each other through the double thickness of the abdominal and uterine parietes; and while the left hand keeps the fundus fixed firmly downwards, the forefinger of the right peels off the adherent mass and forces it through the cervical canal. In the great proportion of cases, I repeat, we can in this manner compel the evacuation of the uterus, and when it fails us our resources are not yet at an end, for,

2nd. We can drag down the cervix from below. The first is the method that has most frequently been employed, and it has this in favour of its common employment that abortions are more frequent in multigravid than in primigravid women; in women, therefore, in whom there is usually a degree of abdominal re-

laxation, which greatly favours its execution. But where the walls are moreresistant, or the patient is so fat that the combined external and internal manipulation fail us, then we must seize one or other of the lips of the uterus—usually the anterior—with a volsellum, double or triple pronged, and slightly curved. One of the blades grasps the vaginal aspect of the front wall of the cervix as high up as the roof of the vagina, the other at a corresponding level within the cervical canal. The uterus is capable of being dragged far down without any injury to its ligaments or laceration in the bite of the volsellum. It may be pulled down with the right hand and kept fixed with it, whilst the fingers of the left pass into the cavity and explore and evacuate it. Or the volsellum may be held in the left hand, or given to an assistant, to keep the uterus depressed, whilst the more familiar right-hand fingers do the intra-uterine work. The cavity of the uterus is thus brought within full reach of the fingers, and we can—and in all those cases of imperfect delivery in the early months we ought to—control the emptying of the cavity from fundus to os.

Whilst the method of gaining access to the interior of the uterus by pressing it down from above is that which has hitherto been ordinarily followed, my own experience leads me to expect that this second method, which I have just described, will largely supersede it. For, first, it is applicable in all cases where the other can be employed, and in some where the rival method is not available. Second, it is less painful, and may be carried out occasionally when there is not time for the administration of an anæsthetic. Third, it saves the expenditure of muscular power demanded of the practitioner, who presses and keeps the uterus pressed down from above only by overcoming the resistance of the abdominal walls. The one circumstance that will enable the bi-manual method to hold its ground is, that we may find ourselves called on to clear out the uterus at a time when we have no volsellum at command, whilst our hands we always carry about with us.—*Edinburgh Monthly Journal*, May 1876, p. 962.

85.—A CASE OF DIFFICULT LABOUR FROM ABNORMAL RIGIDITY OF THE OS.

By Dr. GEORGE CALDERWOOD.

[In the case which forms the subject of the following article, the os uteri, from the commencement of labour in the night and during the whole of the following day, remained in the same rigid unyielding state. The pains during the whole case were strong, but quite inoperative owing to the state of the os. It became evident that some artificial assistance must be given.]

The question was, What was to be the nature of that interference? Forceps were inadmissible, as the os was undilated. The one great object to be attained was to effect its dilatation. The question was how? I have the greatest confidence in Barnes's dilating bags where there is no unnatural rigidity, but in this case I felt they would have been powerless to effect the purpose intended. Another method recommended in these cases is to make small incisions, cutting the os in several directions; but had I done this, I was afraid such incisions would act as centres for subsequent tearing, in any further efforts that might have to be made to effect delivery, and even in the indurated state of the parts by the natural contractions of the womb itself.

Considering all these points, and the case being now urgent, I resolved to dilate gradually with the hand, and then turn and deliver. I accordingly proceeded to do so. One finger was first passed through the os, then after a time a second, and so on till all the fingers were introduced. The constriction at this time was most powerful, and I therefore rested for a considerable time without making any further efforts at progress. At last with some difficulty I succeeded in introducing the whole hand within the uterus, without any perceptible laceration. I then, after waiting a short time, turned and delivered, in the performance of which no unusual difficulty presented itself. The same difficulty that existed to the passage of the child existed to the passage of the after-birth. The womb contracted well, but neither by external manipulation, nor by gentle traction at the cord, could the placenta be removed.

After waiting a proper length of time, I was compelled a second time to introduce my hand through the os, having much the same rigid feel. I went through the same slow process—one finger after another—and found the placenta completely detached from the uterine wall, and readily brought it away. The only object preventing its passage was the rigid os. The patient made a good recovery.

My object in relating this case—and similar cases must be frequently occurring to daily practitioners—is to show what may be done by the hand, provided due caution be used, and above all, provided the operation be not hurried. Milder remedies should first be tried, but should they fail, the gradual dilatation of the os by the hand may with proper care be attempted. In ordinary cases of dilatation of the os, or where it is desirable to produce it with a view to bring on labour, the use of Dr. Barnes's bags is, I believe, the proper course to follow. In such a case as this the choice was betwixt the operation performed and incisions. The latter is, I think, much more likely to be followed by disastrous results than the former

when judiciously employed. The laceration produced in this case must have been very slight, if any at all, as was shown by the same condition presenting itself to the passage of the placenta after the passage of the child. Incisions must have produced a breach of surface, and each incision must have been the seat of laceration to a greater or less extent. In that case absorption of septic matter would have been rendered an easy matter and the danger greatly increased. I ought to mention that her two previous confinements were difficult—instrumental, as I was informed—and it is possible and highly probable that in one or other of these, such changes may have been produced in the neck of the womb as have resulted in its present rigid condition.

To sum up. Use Dr. Barnes's dilating bags if they are suitable for the case—that is, if you have time and if they are sufficient to effect dilatation; if not, use the hand as I have described, in preference to incisions.—*Obstetrical Journal*, Jan. 1876, p. 657.

86.—TREATMENT OF SHOULDER PRESENTATIONS.

By Dr. EDWARD WARREN, Paris.

[The following is taken from a letter by Dr. P. R. Maxson, of Syracuse, New York, giving an account of his treatment of shoulder presentations.]

He says: "I hasten to give you in a condensed form an account of my plan of *treating shoulder presentations*. Gravity is the principle invoked; and I was led to the discovery in 1860 by placing a woman with prolapsed cord on her knees, with her head and shoulders low, as recommended by Dr. T. G. Thomas, of New York, in order to effect its reduction, and finding that while she was in this position an abnormal (abdominal) presentation was spontaneously converted into a normal one. Having reflected on this circumstance, I was induced a few weeks later, when called in consultation in a bad shoulder presentation, to try *position* as a means of rectifying it. I was very anxious in regard to the case, because the lady had lost three children already from 'turning to deliver' in shoulder presentations. Her regular attendant, Dr. G. N. Dox, of Geneva, New York, a physician of attainment and experience, happened to be the *confrère* in whose practice the case of prolapsed cord, above referred to, had occurred; and, instead of 'turning' himself, as had been so unsuccessfully attempted in the lady's previous labours, he sent for me in consultation. Remembering the fate of the other children, and finding this one very large, I suggested the feasibility of correcting this *shoulder presentation* in the same manner as I had corrected the *abdominal* in the first

instance. With his consent I made the effort in the following manner:—I folded several quilts compactly, laying them one upon another to the height of about one foot, and assisted her to kneel upon the quilts, with her head and shoulders resting upon the bed, and her face forwards, so as to bring her body to an angle with the bed of nearly 90 degrees. I then pressed my hand gently against the shoulder, which readily receded, until I was enabled to clasp the vertex with my fingers, and with the assistance of the next pain to so ‘engage’ it, that, when the patient was placed upon her left side and the quilts removed, a perfectly natural presentation presented itself. In a few hours the labour terminated in the delivery of a healthy boy, weighing ten pounds. Only a few moments were occupied in the process, and subsequent experience convinces me that *shoulder presentations* can generally be converted in this way into *natural* ones, without a resort to ‘turning,’ and with no risk for the mother or the child.”

I would add that this method of treatment has received much attention in America, and that no inconsiderable amount of testimony has been accumulated in support of its efficacy. Dr. Maxson also informs me that he submitted his views in this regard some years since to Sir James Simpson, and that they were cordially endorsed by that distinguished gentleman.—*Lancet*, Dec. 18, 1875, p. 897.

87.—LACERATION OF THE PERINEUM.

By Dr. JAMES YOUNG, Vice-President of the Obstetrical Society of Edinburgh.

The treatment of laceration of the perineum is one of the most important questions that can come under the notice of the obstetric surgeon. I propose to cite two cases, illustrative of the benefit of treatment by the interrupted suture, more to elicit discussion than for the purpose of bringing forward any new matter. The second case is almost unique in regard to the extent of the rupture, and likewise the result.

Case 1.—Some twelve months ago I was in attendance upon a lady in Maitland Street. It was her first confinement. The labour was protracted and difficult, requiring the use of the short forceps. I used very considerable strength in traction, but without the pendulum movement, and failed to extract the head. I sent for my friend Dr. Charles Bell, who kindly came to my assistance. The forceps were again applied, and the pendulum motion, with powerful traction, was successful in delivering the patient of a fine, large, healthy boy; the perineum was torn, but not through the sphincter ani. The wound was carefully sponged, and brought together with the inter-

rupted suture in three places in less than half an hour after the accident. The result was most satisfactory, and the patient made an excellent recovery. The ligatures came away in four days, and the wound was absolutely healed in ten, at every point. The usual rules were enforced.

Case 2.—To this one I would direct attention. On the 28th of June, 1875, I was summoned to see Mrs. M., æt. 35, a primipara. At 6 p.m. the os uteri was small (size of a shilling), although the patient had been in labour for twelve hours. I was again called at 6 a.m. next day, when I found the first stage almost over, and the head presenting in the occipito-anterior position. The woman had been twenty-four hours in labour, and as I considered it unjustifiable to leave her longer, I sent for the forceps. The vagina was hot, and the pains were becoming feeble. While under chloroform, I used steady traction during each pain, allowing the external parts time to dilate slowly. Notwithstanding every care, the perineum ruptured right along through the sphincter ani, and into bowel three inches, my whole index finger easily passing from bowel into vagina. When the placenta was expelled, and the uterus contracted, the wound was carefully sponged. The anæsthesia being maintained, the torn parts were brought together with the interrupted suture. Seven ligatures were used, which had been dipped in carbolic oil, and the wound was left in perfect approximation. The urine was drawn off every twelve hours. The thighs were tied together, and by the administration of opium, the bowels were confined for six days. No local dressings were used. The patient made a perfect recovery; the wound healed throughout at every point; and on the fourteenth day she was left to her own care. Several weeks ago, I examined the patient by placing one index finger in the bowel, and the other in the vagina, and found the recto-vaginal septum complete. Let me here mention, in connection with her history, that when Mrs. M. was married, I understood that perfect sexual intercourse was precluded for some months in consequence of the extreme rigidity of the vagina, and four years elapsed ere this child was born.

Remarks.—1st, Causes of laceration of the perineum; 2nd, Means of prevention; 3rd, General rules of treatment. Many obstetricians will agree with me in saying that, in numerous primiparous cases, the perineal portion of the vaginal mucous membrane is frequently ruptured, and only heals by leaving a sulcus, which rather favours than hinders future labours. Among the causes of perineal rupture might be enumerated, 1st, When the age exceeds *thirty* years; 2nd, Cases where the head of the child is very large; 3rd, Malpresentations; 4th, A small or deformed pelvis; 5th, The use of forceps; 6th, A rigid

perineum ; each cause operating more especially in primiparous women.

Prevention.—I generally adopt the plan of having lard, butter, or cold cream, rubbed over the perineum during the extrusion of the head forwards. 2nd, Gentle dilatation of the external parts with the finger may be adopted during each pain; 3rd, Slow traction, when the forceps are used, and only during each consecutive pain; 4th, The application of the hand in supporting the perineum during strong expulsive pains; and when forceps are employed, during the delivery of the head, the left hand may be spread over the distended surface of the perineum.

Treatment.—In simple cases of laceration of the mucous membrane of the vagina, or even where the margin of the sphincter vaginæ is torn, the only treatment necessary is mere cleanliness and sponging. I extremely deprecate, in any case, the use of bandages, pads, or plasters, as being more irksome to the patient than useful. In severe perineal rupture, as in Case 2, the immediate closing of the wound is of paramount importance, so as to secure healing by the first intention. The interrupted suture of carbolised cat-gut should be used; and the entire rupture must be brought into exact approximation. Careful and frequent sponging must be attended to by the nurse, to avoid any irritation from the lochial discharge. The urine must be drawn off every twelve hours; no dressings applied; the patient kept in the horizontal position; the thighs kept together; and the bowels must not be allowed to move for six days.—*Edinburgh Medical Journal*, Jan. 7, 1876, p. 607.

88.—REPORT ON THE TREATMENT OF RUPTURED PERINEUM.

By THOMAS SMITH, Esq., Assistant-Surgeon to St. Bartholomew's Hospital.

[The treatment of ruptured perineum by postural means alone, during the fortnight or three weeks succeeding labour, although so generally adopted, unfortunately often fails. It is better at once, or within a day or two to insert some sutures—it will lead to less discredit in the end.]

Where for any reason the rent remains unclosed after recovery from childbed, the operation should certainly be postponed until the process of uterine involution is complete, and normal health has been regained. It is prudent to delay operating until the cicatrisation of the torn surfaces has taken place, for without doubt the freshly-pared mucous surfaces would be more likely to unite quickly and soundly than surfaces formed of the deeper layers of granulations, where possibly the new tissue had scarcely

completed its perfect organisation. Though it is certainly an advantage that all vaginal and uterine discharge should have ceased before any operation is performed, yet the presence of leucorrhœa is no bar to success should other circumstances be favourable.

In a case under my own care, in which the leucorrhœal discharge could not be arrested by treatment, the operation was performed with a good result, though the discharge gave some trouble during convalescence.

It need only be mentioned that no operation should be performed during lactation; while, as a rule, it may be left to the patient to decide whether she will wean her infant, and submit to an early operation, or bear her troubles until the natural termination of lactation. The only reason that can be urged against this course is the possibility that during the postponement of the operation prolapsus uteri may occur. This consideration, I think, should have some weight in determining the date of operation. I have recently cured a patient of a rent perineum who suffered from complete procidentia uteri. She was only twenty-four years of age, and the rupture had existed a year and nine months. In her case, at least, one would think that the uterus had begun to descend before the period when lactation usually terminates.

The treatment of ruptured perineum, when undertaken immediately after delivery, resolves itself simply into readapting the opposite surfaces of a wound which are in a condition most suitable for union: they are freshly torn, they exactly fit one another, and the parts are so lax that there is no tension on the sutures. The only point for consideration is the selection of the best material to use for sutures, and the best means of introducing the sutures. Soft silver wire is, as I believe, the very best material, on account of its cleanliness, the very slight irritation it causes in the tissues through which it passes, its inelasticity, and the ease with which it is withdrawn.

Soft iron wire is only inferior to silver on account of its tendency to become hard and corroded when left for some time in a wound, and thereby giving some pain in its withdrawal. Silk and twine are less suitable as materials for suture, as they cannot be long retained without exciting suppuration.

Catgut, whether carbolised or not, is to be rejected altogether for the operation under consideration. If carbolised, it is liable to undergo solution in the secretions of the wound; while ordinary catgut is uncleanly, and irritating to the tissues.

Any form of suture-needle may be employed that will carry the suture through the whole depth of the wound, entering the skin a full inch from one margin, and emerging at the same distance from the opposite. Least painful to the patient is

some kind of tubular needle, through which the wire may be pushed, such as Startin's or Matthieu's needle. Just as efficient, though causing more pain, is the large sickle-shaped needle, fixed in a handle, having an eye at the point. If such an one be used, it should be thrust through the soft parts without the suture, the latter being threaded when the needle-point emerges from the skin, having completely transfixed both sides of the rent: the needle, as it is withdrawn, will leave the suture in position.

For very slight ruptures no such formidable instrument is required as the one above mentioned, since the curved needles that are ordinarily carried in a pocket-case will suffice.

In operations performed within a few days of delivery, unless the rent lay open the rectum, the sutures may be fastened by simple twisting; and unless they excite irritation, these may remain *in situ* for a fortnight. When the rectum is laid open, it is more prudent to fasten the sutures to leaden plates, buttons, or pieces of bougie, so as to bring the parts together after the manner of quilled sutures, two or three superficial interrupted sutures being also inserted.

It will be convenient to consider here two questions of great importance in the after-treatment of operations for ruptured perineum—

1st, When should the sutures be removed?

2dly, How should the bowels be managed?

With regard to the removal of the sutures, Mr. Baker Brown (to whom belongs the credit of introducing the essential details of the operation now generally adopted) makes the following statement:—"The deep sutures should be removed on the second day; in about forty-two hours is the best time. On the sixth or seventh day, the interrupted sutures may be taken away."

Mr. Erichsen states as his opinion "that the deep sutures should be left in for three days; in some cases they may even be retained for ninety-six hours. If any suppuration occur along their track, they must at once be withdrawn. The superficial sutures should be left in as long as they produce no irritation."

Mr. Bryant on this point remarks—"The deep sutures should be removed on the third or fourth day, the superficial on the seventh."

Mr. Hutchinson, on the other hand, says—"The sutures should not be removed until the sixth or seventh day."

My own experience quite agrees with all the above-mentioned opinions, it being remembered that the three first-named surgeons are speaking of quilled sutures made of silk or twine, and fastened to pieces of bougie, stick, or thin ivory rods, while

Mr. Hutchinson refers to wire sutures fastened to metallic plates.

There can be no doubt that quilled sutures of twine or silk, fastened to pieces of cane or bougie, cannot be left in longer than forty-eight hours without running a risk of exciting suppuration in the suture tracks, and causing sloughing of the integuments beneath the quills or bougies.

It is, on the other hand, quite certain that, as a rule—if the deep sutures be metallic, and if they be fastened to flat plates, very wide bars, or large studs or buttons—they can be allowed to remain in for a week or more without injury to the soft parts, either from sloughing of the integuments or from suppuration in the suture tracks.

The deep sutures should not, as a rule, be left in more than a week, since the tendency of this kind of suture is to cut its way out from the deepest part of the wound towards the surface; and in this way, doubtless, a cavity may be formed between the rectum and vagina which may give rise to a recto-vaginal fistula.—*St. Bartholomew's Hospital Reports*, 1875, p. 215.

89.—FOLDING SHORT FORCEPS.

By W. DRAPER, Esq., Surgeon to the York Dispensary, late Resident Obstetric Officer to the Middlesex Hospital.

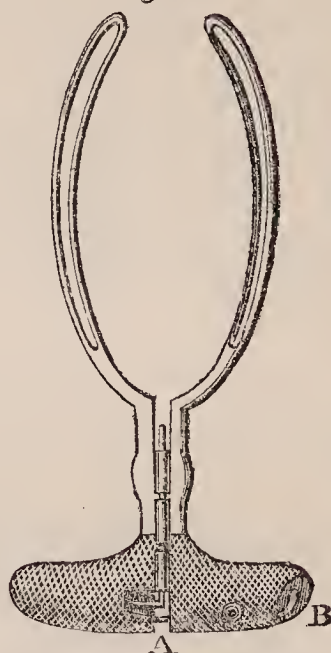
The forceps about to be described take the name of *folding* forceps, in consequence of the two blades of the instrument being so constructed as to fold one within the other in such a manner that both are introduced at the same time. In the accompanying illustration, Fig. 1 shows the forceps closed for introduction, and Fig. 2 exhibits them open.

To the invention of the general principle of the forceps (that of the simultaneous introduction of the blades one within the other), I do not for one moment lay the slightest claim, for I believe Dr. Vacher was the originator of the idea. My forceps are a modification of Dr. Vacher's, and, after a fair trial of both, I venture to say that my instrument is a considerable improvement upon the original design. A short description of both forceps will best show the relative merits of each.

In Dr. Vacher's forceps the handle is small and spindle-shaped, affording but a slight and uneasy hold for the hand; in the middle of the handle there is a projecting knob, the proximal end of a hinge bolt; this knob forms a most inconvenient obstacle to the hand when the handle is grasped. Then, again, at one end of the handle there is an awkward lever, connected with a catch which locks the handles together. This lever projects considerably beyond the handle, and there-

fore forms another object of discomfort to the operator's hand; moreover, the projection is very apt to catch the bed-clothes, or the patient's dress, the result of which would be that the lock would be set at liberty, and the blades would lose their hold of the foetal head.

These matters may, at first sight, appear trivial, but as a matter of fact, practically, they are of no small importance both to the comfort of the operator and to the effectiveness of the instrument.

Fig.1.*Fig.2.*

In describing these points I do not at all wish to detract from the value of Dr. Vacher's forceps, my only aim being to remedy defects, and to introduce a more perfect and convenient instrument.

As to my own forceps. In the first place, when open, the handle, which is made of ebony, is of the form depicted in Fig. 2, and fits the palm of the hand and the fingers perfectly, and a good firm grasp of it may be obtained, and maintained, without discomfort. This handle is neither clumsy nor weighty. The proximal end of the hinge-bolt, which connects the neck of the forceps, is bent at right angles, and being let into one side of the handle, it is quite concealed, yet, if it be necessary to separate the blades during an operation, this separation can be quickly effected by a very simple movement. Then, again, my spring lever in connection with the catch which locks the two parts of the handle together, is enclosed within a thumb depression at one extremity of the handle; the lever, therefore, and the parts upon which it operates, are

quite out of the way, yet by placing the tip of the thumb in the depression, the spring readily acts.

The blades of my forceps are made as light as possible, consistent with a moderate degree of strength, and as the instrument is applicable only in cases in which no great amount of force is required, it is unnecessary for its blades to be either stout or heavy. The pair of forceps which I now have in use weigh only 8 oz. I consider, however, they might, with advantage, be made still lighter. My instrument, too, is electroplated, the great advantage of which being, that the hinge-joint upon which the blades revolve, and the spring lever and catch, are not liable to become rusted by the secretions, &c., and their action thereby interrupted; and, further, the plating insures the general cleanliness of the instrument. The forceps are so small and portable that they may be conveniently carried in the breast pocket.

A word or two as to the way in which the instrument is applied, and the conditions under which it is most useful, may not be out of place.

The forceps being *folded*, with the convexity of one blade lying within the concavity of the other (as Fig. 1), the *two* blades are simultaneously introduced exactly in the same way that one blade of the ordinary short forceps is introduced; this having been done, one-half of the cross handle is held *in situ*, while the other half is gently separated from it, the result of which is that the small hinge-joint, connecting the shanks of the blades together, is acted upon, one blade glides from under the other, over the foetal head, and, with a little management, this revolving blade is easily got into position opposite to the fixed blade: the two halves of the handle now come into apposition, when, by means of the spring catch, they are firmly locked together, and form the cross handle shown in Fig. 2.

If, during an operation, it be desired to take off the forceps, it is necessary simply to withdraw the hinge-bolt, when the two blades are at once separated and set at liberty.

A difficulty may probably be apprehended by some in getting the instrument to act properly in a contracted tense vagina, in a narrow pelvis, or in certain positions of the foetal head, but the room taken up by the blades is so small, and so flexible are they, that no such difficulty *really* presents itself.

The forceps are *applicable* in all that numerous class of cases in which slight assistance is indicated to terminate tedious, but otherwise natural, labours—in fact, they may be employed in all cases in which ordinary short forceps are applicable. They are, I need hardly say, available only after the head has passed through the pelvic brim.

There is one more point worthy of note—viz., that these forceps may be used, if it may be deemed desirable, under the bedclothes, without the knowledge of the patient or her attendants; and it is, I hold, often a matter of no small importance, that a sensitive and delicate patient should be saved the shock and natural dread occasioned even by the most gentle mention of the use of *instruments*.

Finally, I am satisfied, that if these forceps be carefully and judiciously employed, they are capable of being a great and efficient aid both to the patient and to the obstetrician.—*Obstet. Journal, Feb. 1876, p. 715.*

[Mr. Draper's forceps are made by Arnold and Sons, 35 and 36, West Smithfield, London. Dr. Vacher's forceps are described and figured in *Retrospect*, vol. lxxvii, p. 272.]

90.—AGAINST THE PENDULUM MOVEMENT IN WORKING THE MIDWIFERY FORCEPS.

By Dr. J. MATTHEWS DUNCAN, Edinburgh.

[Most men who use the midwifery forceps frequently must perceive that the side-to-side movement of the blades, as recommended and usually practised, is not productive of good, but harm.]

In the following remarks I shall make reference only to that pendulum movement from side to side, which alone is, so far as I know, recommended in these latter times. The pendulum movement in a sagittal direction, as recommended by the early describers of the forceps operation, is still more open to objections than the former.

In describing or defending the pendulum movement, two great points are made: first, that it is analogous to, or identical with, that of a lever and double rack; and, second, that by resorting to it there is an economy of force.

The lever and double rack hypothesis may be considered first. It is difficult to dispose of it only because it is so flimsy that it is impossible to get hold of it. There is no toothed rack on the wall of the pelvis. There is no roughness to take the place of a toothed rack. Were there any such teeth or roughness, the worst use that could be made of them would be to make them assume the function of a rack. The accoucheur should strive to advance the head as smoothly and with as little impression as possible on the walls of the pelvis. Further, there are no teeth or roughnesses on the foetal head to fit into the teeth of the supposed rack.

Without the lever and double rack hypothesis substantiated, the movement must be useless; for it is, and must be, done to no purpose. It is conceivable that the head may be seized with

such a degree of firmness by the blades of the forceps as to be moved or made to revolve as on a pivot; but such movement, without the additional force required in simple and successful traction, uncomplicated by the movement, would be of no avail. It would not advance the head. Without the lever and double rack, it would only cause revolutions as on a pivot—it would not produce progress.

Let us imagine that the accoucheur, dragging to one side, produces advance of the opposite side, which he maintains by continued dragging, while he makes the oscillatory movement in order to drag to the other side. In this way he may make the head advance while using an oscillatory movement. Every one knows that this is easily done. But then every one must also recognise the utter inutility of the movement. It is the imitation of the action of a lever and double rack without a trace of its utility. An unnecessary movement like this has great disadvantages. The pressure exerted, and the traction force used, are probably greater, certainly not less, than if simple traction were exerted to produce the desired result. Pulling the head down at one side and then at the other, and so advancing, is merely an injuriously complicated way of producing simple progress. It produces no evasion nor diminution of the difficulty to be overcome, while it has concomitant and avoidable evils.

In answer to this reasoning one might refer to an analogy in the way in which a cork is sometimes extracted from a bottle without using a corkscrew. But a study of this analogy only confirms the argument. For the cork would be better, and on the whole easier, extracted by simple uncomplicated traction as by a corkscrew. Besides, the cork and the bottle mutually exert such friction force as prevents retraction of one side if advanced, just as if the bottle were a rack and the cork the lever; and such is in no sense the case with the foetal head and pelvis. Lastly, this oscillatory advance of the cork is only sought when power fails to produce a direct advance, and, in the case of the forceps, there is never deficiency of power; for, as we have already said, the power applicable by the accoucheur is practically boundless, or, in more sober terms, it is greater than he can safely apply.

The idea that there is any saving of force, so far as pressure on the mother's and child's part is concerned, by resort to the oscillatory or pendulum movement, is such that I cannot argue against it. The question involved is purely mechanical and of extreme simplicity. It is this: a mechanical difficulty in bringing a child's head through a resisting passage has to be overcome; further, the difficulty is not to be evaded by changing the position of the child's head; on the contrary, that position may be

supposed to be the most favourable for facility of propulsion. Now, can any oscillation or other imaginable movement diminish the mutual, and, in this case, injurious pressure or force required to produce advance? The question requires no answer. The supposition is absurd. Moreover, the absurdity is not less, whatever phase may be given to the hypothetical advantages of the pendulum movement. A certain amount of work has to be done; the head has to be advanced against resistance that must be overpowered if the effort is successful. Direct uncomplicated traction does the work in the simplest way, and no complication of it by pendulum movement or other can diminish the amount of work expended below that required by simple traction. The complication of simple traction may, however, increase the expenditure of force to a great extent.

The pendulum movement necessarily involves an injurious amount of pressure and consequent friction, in all cases, between the parts of the head to which the blades of the forceps are applied and the adjacent maternal structures. No doubt this friction is in most cases so slight and temporary as to be of little moment. But in some cases, when the resistance to progress arises from tight and undilatable soft parts, it may be very injurious. The most important forceps cases, however, are those where the obstacle to progress arises from hard parts; and, of these cases, the most frequent and characteristic are those of simple narrow or flat pelvis. In such cases the head has to be slowly dragged and perhaps moulded between the promontory of the sacrum and the pubic bones. Now, here, the pendulum movement involves special evils and dangers; for by it there is necessarily produced, besides the trivial friction, which is most extensive at the points where the blades are applied, a violent and powerful squeezing of the soft parts between the head and the opposing pelvic bones, on which the head works. This combination of wriggling and squeezing is altogether unnecessary, and must greatly aggravate the necessary or unavoidable mutual pressure, which is bad enough.

If, for the carrying out of the plan of pendulum movement, the forceps is made to compress the head so strongly as not to slip on it, which mode is probably regarded as desirable, then the points of the forceps, and especially the point of that blade which is on the side of the head towards which the movement is given, will exert a specially powerful, and certainly undesirable, amount of pressure on the parts of the child's head or face which they touch. If, on the other hand, the blades do not press the head with such firmness as to obviate a to-and-fro motion of them on the head, then the scalp will be liable to be much injured, and its surface abraded; conditions which are often observed as the result of this kind of proceeding.

There is, in the mechanism of delivery, whether natural or morbid, nothing analogous to this artificially produced oscillating or pendulum movement. Nature pushes a foetal head through an obstructing passage by force, which produces, or may produce, on the one hand, dilatation or laceration of the passage; and, on the other hand, various kinds of changes in the shape and size of the foetal head. Our best guide in forceps cases is the process of nature; and it is probable that any future improvements in the working of the instrument will be the consequence of closer and successful investigation of the mechanism of labour in these difficult cases. The pendulum movement, in working the forceps, does not advantageously increase the power of the instrument to produce desired results.

The use of the forceps is to contribute, by artificial pulling, to the strength of the natural expulsive efforts which push. To this traction, judiciously applied, the practitioner should confine himself. The oscillatory movement will contribute nothing to the forward traction, and it is the forward traction which alone is desirable.

In corroboration of these theoretical views as to the injuriousness and inutility of the pendulum movement in the working of the midwifery forceps, I might appeal to the extensive experience of myself, and of many other practitioners. But such appeal can only be held as evidence sufficient to show that the pendulum movement is not necessary. It affords no evidence that using it or abstaining from it is the preferable plan. And I cannot imagine any method available at present whereby the results of experience can be made suitable for the final settlement of the matter.

Both plans are used by good practitioners. Traction without oscillation is simple, effective, and in accordance with the method of nature's own efforts. Traction with oscillation is complicated: and many theoretical and practical objections may be made to it.

Other objections might be adduced against the pendulum movement, and some are candidly stated by authors who recommend its adoption. I have confined myself to the discussion of its supposed utility and inevitable evils. The only advantage which I can conceive it to possess, is one to which no one will avow his indebtedness, for it would be an admission of culpable want of knowledge, and consequent unjustifiable practice. A practitioner ignorant as to the proper direction of pulling may, by this motion of the forceps while extracting, fall accidentally upon the right direction, and thus do some good by mere luck, and at much risk, which should have been done intelligently, and without avoidable risk. It

is, perhaps, in order to ensure this kind of possible success, that some authors recommend the movement to be not pendulum-like, but rotatory, or in the grand so-called tours.—*Edinburgh Medical Journal*, Feb. 1876, p. 683.

91.—REMARKS ON THE INEVITABLE AND OTHER LACERATIONS OF THE ORIFICE OF THE VAGINA, AND NEAR IT, IN PRIMIPARÆ.

By Dr. J. MATTHEWS DUNCAN, Edinburgh.

In the outset I desire carefully to point out that anatomically the orifice of the vagina and the obstetrical perineum have no direct connection. They are quite distinct parts, removed from one another by the structures forming the fossa navicularis. It is the orifice of the vagina which I declare to be invariably injured in the natural labour of a primipara. The perineum does not always escape laceration, but it frequently does so. It is not only anatomically that the orifice of the vagina is distinct from the perineum; it ought to be distinguished practically. When the head is on the perineum, or has passed beyond the outlet of the ligamentous pelvis—"out of the bones," as I have heard midwives call it—it is generally described as retained by a rigid perineum. But this description is plainly often inaccurate, for the perineum has yielded as much as can be expected or wished of it, and yet the head is not born. This condition is seen almost exclusively in primiparæ, and it is the orifice of the vagina, not the perineum, that is undilatable or rigid. [In such cases, it is habitual to deliver by the finger in the rectum pressing on the glabella in a proper direction, to force the head through the orifice of the vagina. This proceeding I object to, as I have elsewhere described, preferring the safer and equally efficient pressure through the skin in front of the coccyx or in the region of one or other sacrosciatic ligament.] To nearly complete the proof of my statement I have only to add, that in such cases the birth is often completed without injury of the perineum, the frænulum or fourchette even remaining entire. When the undilatability of the orifice and lower part of the vagina is great, and demands interference by the practitioner before the head has descended so far as to be amenable to pressure on the glabella, the forceps is applied; and, under such circumstances, I have often delivered without any injury of the perineum resulting, though certainly not without any injury of the orifice of the vagina.

After a natural labour the pudenda lose their characteristic appearances. The hymen is partially or completely destroyed. If the orifices of the ducts of Duverney's glands are not discoverable, it is impossible to say exactly where any part of the

vaginal orifice is, except when distinct remains of hymen are seen. The distinction of the fourchette or posterior margin of the vulvar outlet from the margin of the vaginal orifice cannot now be made with any exactness. The labia majora have shrivelled, and the position of their projecting posterior piers is ill defined. If in a multipara the head is obstructed by the undilatability of the orifice of exit from the woman's body, then it cannot be said that the vaginal orifice is rigid and obstructing, for that part is not recognisable. It is the vulvar outlet that, by its undilatability, impedes progress. But the vulvar outlet from the genital passage is not the same thing as the vaginal orifice.

The lacerations of the vaginal orifice, as distinguished from those of the perineum, are very important. They may be the seat of pyæmic infection. Those in the anterior part of the vaginal orifice often produce hemorrhage, which is occasionally, but rarely, even fatal, especially if the tear extends to the clitoris. Those on the posterior part of the vaginal orifice may be the commencement of greater lacerations, and they may be not only the commencement, but also predisposing causes of further laceration.

The lacerations of the vaginal orifice are not only to be distinguished from those of the perineum, but also from the other lacerations of the vagina. These are splits of the vaginal tissues of greater or less depth. They often are quite distinct from the upper vaginal lacerations connected with laceration of the cervix uteri, just as they are distinct from the lower vaginal lacerations connected with the vaginal orifice. They are produced by over-distension of the canal, and, as might be expected, are longitudinal in direction.

It would appear that, in the Darwinian progress of the species, the head of the foetus has increased in size more rapidly than the orifices and passages through which it has to come have increased in size or dilatability. For it can scarcely be supposed to be a final arrangement that the cervix uteri should be torn so often as it is in giving passage to the child; and the same may be said of the lacerations of the vagina, the vaginal orifice, and of the perineum.

When, in a primipara, the head has distended the perineum, it is arrested by the vaginal orifice. During a pain, the head is propelled against the orifice in the line of its axis, or nearly so. A part of it acts on the orifice as a wedge projecting through it, and helping to dilate or distend it, or to burst it. If the orifice is unyielding, the pains push it as a whole more and more forwards, and among other consequences of this there is great elongation of the perineum. All parts of the circle are pushed forwards, the fourchette most of all. But even

anteriorly there is great tension, and some degree of forward movement.

In this condition of matters, it is not one part of the orifice that is stretched, but the whole of it; and considering the shape of the foetal head, it is at every point probably not equally, but nearly equally, stretched. The force tending to tear it, is acting nearly uniformly at all points of the circumference of the orifice. The condition of parts may be imitated by pushing a four-inch globe through a circular orifice of a considerably less diameter, in a sheet of indiarubber. The sides of the orifice are stretched in a longitudinal direction, while they are distended.

In this crisis, the distended orifice of the vagina in the primipara does not yield, at the same time retaining its entirety. It yields, and at the same time is invariably lacerated; and I know of nothing that can modify the tearing, unless such delay as may ensure all the dilatation possible before laceration commences; and such modification of the direction of the propelling pressure as may perhaps diminish the laceration of the weak posterior parts. When laceration has begun, further enlargement of the orifice is comparatively easily attained by further laceration. Whatever other laceration takes place, there is always some at or near the mesial line posteriorly. As I have already said, I have often seen primiparæ delivered, and even by forceps, without any perineal laceration, the fourchette being entire; but I have never in such a case separated the labia and examined the vaginal orifice without finding a laceration of its posterior part.

It is quite common to hear assertions of the complete absence of laceration in primiparæ, but I have never been satisfied that, in the cases referred to, a sufficiently careful examination has been made. To do it one requires a good light, an assistant, and a sponge.

It is scarcely necessary to remark, that the laceration of the vaginal orifice is frequently the first step to laceration of the fourchette or of the perineum more extensively. But all lacerations of the perineum are not mere extensions of tears beginning anteriorly. Lacerations commencing in the vaginal orifice or in the fourchette lead to many, probably to most, perineal lacerations, which may therefore be called secondary. But there are primary and essential or independent perineal lacerations, as is proved by the occurrence of central rupture.

The inevitable posterior laceration of the vaginal orifice is not the only one that occurs. It is often alone, but sometimes there are others. There is sometimes laceration of the side of the orifice anteriorly, where the parietal tuberosity has pressed, and this tear may extend into the adjacent nympha, and bleed

freely. I think the left side is more frequently the seat of this tear than the right; and this is explained by a study of the mechanism of the delivery in a first or left occipito-anterior position. In it the right parietal tuberosity is born before the left; and when the left is passing, a greater ploughing pressure is exerted on the vaginal orifice than was exerted by the right tuberosity when it passed. This is a consequence of the greater dimensions of the part passing when the left tuberosity is in the orifice than of the part passing when the right was in the same orifice.

Sometimes irregular lacerations take place, little detached flaps being left hanging. These are probably produced by the tension at the orifice in a longitudinal direction, aided, perhaps, by a slight degree of friction, producing tears in a circular direction, and the subsequent detachment or pushing off of tags.

But the most important tears of this part are at its anterior margin, this special importance arising from the hemorrhage which they occasionally produce. The tear takes place before the child's head is born, and the event may be diagnosed, if a sufficient interval of time elapses, by the period of its occurrence and its anterior position at the occipital region of the foetus, blood being found in that situation. The exact mechanism of this tear I have not satisfactorily made out. I believe it sometimes affects only the side of the urethra, and then it is truly a laceration of the vaginal orifice.

The important laceration of the vestibule, extending backwards to the side of the urethra, and forwards to the clitoris, is not common, and difficult to investigate, and on this point my observations require further confirmation. I meantime designate this a laceration of the vestibule or triangular part bounded by the nymphæ, clitoris, urethra, and anterior portion of the orifice of the vagina. If all this be correct, then this vestibular injury is not a laceration of the vaginal orifice, and is rather analogous to the laceration of the perineum posteriorly. When the vestibule is distended and developed anteriorly, as the anterior part of the perineum is posteriorly and subsequently, the place of the fourchette is taken by a fold of the vestibule. The edge of this vestibular fold tears, and the tear may extend forwards to the clitoris, or backwards by the side of the urethra. I must state that I have verified these observations by actual investigation, except the paramount one of the actual occurrence of the tear, which I have not seen at the moment of its production.

As to the important point, the order of time in which the lacerations of the vaginal orifice occur, I can give no good statement. Only it is quite sure that they all take place before

the perineum is torn as an extension of the laceration of the vaginal orifice. When the perineum is so torn, the extreme tension of the whole vaginal orifice is of course annulled, and the liability to further laceration of it removed.

The inevitable laceration of the orifice of the vagina in natural labour in primiparæ takes place at or near the mesial line, in the posterior border of the orifice. It extends longitudinally, passing posteriorly along the vaginal wall, and anteriorly into the fossa navicularis. The laceration goes through a triangular mass of tissues, whose apex is the orificial margin, the part first torn, and whose base or longest side is the line of the middle of the bottom of the wound made. It is interesting during labour, to watch the progress of this tear. The first little laceration or nick may be felt to be gradually increased till the full extent of the laceration is produced. If the head retreats, the wound may be felt to be of some depth, its sides collapsing; but if it only partially retreats, some tension remaining, and the finger is passed to examine, then the wound has no depth, but presents an extended, lozenge-shaped surface, whose margins may be recognised, and whose four angles are placed, one just behind the fourchette, one higher up in the vagina in the mesial line, and the other two at the now remote but formerly conjoined parts of the margin of the vaginal orifice at which the split or laceration began. After delivery the wound resumes comparatively small dimensions; but by proper manipulation it may still be shown, by separation of the labia (in imitation of the distension by the foetal head), to have the characters above described as present during the passage of the head.

Before concluding, I shall enter briefly on the causes of this kind of rupture, although their consideration does not form an essential feature of the subject, because the causes are nearly identical with those of ordinary perineal ruptures.

There is no doubt in my mind that, in certain cases, there is what may be called rottenness of tissue, which destroys the power of the tissues to resist laceration or bursting. In some women, and occasionally at least very markedly in the syphilitic, this condition is very easily demonstrated. It is a condition also of many inflamed tissues, and this is exemplified in the perineum.

The element of time is important in the study of causes, for there is general consent that a part rapidly dilated may give way, while the same part slowly dilated may be induced to yield and dilate without tearing or giving way. This is almost a truism, illustrated, as it may be, in all tissues, living or dead.

The element of time cannot be fully considered without simultaneously attending to the element of force or pressure.

Of course, all tearing is effected by a pressure superior to the resistance. A pressure slightly superior to the resistance may only prove itself to be so after many attempts and considerable lapse of time, and, it may be, without laceration, except what is inevitable; whereas a greater pressure may rapidly overcome the resistance, and will probably do so by producing one or more lacerations.

The element of force or pressure cannot be fully studied without taking into consideration the child's head or the body propelled against the resistance. Now, it appears to me, that the head must be regarded as a blunted or pointless wedge. A point is not required, because there is already a passage which needs only farther dilatation, not new formation. The wedge-shaped head will be more efficient in proportion as it is sharper; and this condition of sharpness will be increased by three circumstances that are subjects of frequent observation. First, it is evident that the overlapping of bones will increase the sharpness and power of the wedge. Second, the smaller the foetal head, the sharper its wedge-like shape. This may offer an explanation of the circumstance demonstrated, or nearly so, by Hecker, that the mere size of the head is not in direct proportion to the frequency of laceration of the perineum. Thirdly, the recent caput succedaneum may act as a wedge on parts not otherwise subjected to any distending pressure, and it may also increase the sharpness of the already previously acting wedge-shaped portion. Of course, in this theory it is taken for granted that the edge of the caput succedaneum is below, or acting on the part to be dilated. Lastly, the wedge-like shape will be changed by projections such as the parietal protuberances, which will diminish the sharpness of the wedge so far as these particular parts are concerned, and increase the likelihood of laceration by these parts.

The direction of the propelling power is also to be taken into account, and may be regarded as always producing greatest pressure on the posterior part of the passage, the part which has to undergo the most extensive dilatation. Referring to the commonly used term, circle of Carus, we may point out that this direction of special pressure is a matter of course; as it is true of all curvilinear motion, the counter pressure on the convex side being required to produce the deviation from a straight course.

The part that is weakest will always, *cæteris paribus*, be the first torn. The actually weakest is probably the posterior mesial part, and, as has just been stated, it has to endure the disadvantage of being subjected to greatest pressure.

If a laceration is inevitable, treatment to prevent it can be of no avail. But all the lacerations of the orifice of the vagina

are not inevitable; and that one which is so, may be treated with a view to prevent its extension beyond the inevitable degree.

Two important elements in the causation of laceration are susceptible of modification with therapeutical objects by the practitioner, namely, time and direction. The accoucheur can prevent the precipitate expulsion of the child, and its attendant evils. He can, by supporting the perineum, modify the direction of its advance, and resist any undue pressure posteriorly or inferiorly arising from the curvilinear motion of the foetus.—*Edinburgh Medical Journal*, March 1876, p. 822.

92.—ON PUERPERAL FEVER.

By Dr. WILLIAM O. PRIESTLEY, President of the Obstetrical Society of London.

[The following, which is part of the presidential address, is to some extent a review of the opinions expressed on the subject of Puerperal Fever during the late discussion on that subject by the Obstetrical Society of London.]

Formerly, two opinions were held concerning the pathological nature of puerperal fever. Puzos, Levret, Hamilton, and White regarded it as an idiopathic putrid fever. Armstrong, Mackintosh, and Campbell maintained that it was essentially a local inflammation, of which the fever-symptoms were merely a sympathetic consequence. Boyer, Le Gallois, and Cruveilhier endeavoured to show the analogy between the condition of a woman recently delivered and any other patient who was the subject of surgical injury, and to point out the similarity of the morbid affections to which both were liable. Cruveilhier, indeed, compared the uterus after delivery to an amputated stump, inasmuch as its muscular walls were laid bare, and at the placental spot where the gaping orifices of divided vessels. Subsequent research has proved that the uterine walls are not so absolutely denuded as M. Cruveilhier supposed; but there is no doubt as to the presence of large open channels at the placental spot, through which deleterious matters may gain a ready access to the general circulation. Following out this idea, Sir J. Y. Simpson in 1850 wrote a memoir on the analogy between puerperal and what he called surgical fever—a term much objected to by some surgeons.

Probably the best essay on puerperal fever which has appeared during the present generation is that by the late Dr. Robert Ferguson, one of my predecessors in the Chair of Midwifery at King's College. Dr. Ferguson attributed puerperal fever to a vitiated state of the fluids of the body, and regarded all varieties of local lesion as consecutive to vitiated circulation. He

pointed out, what is now generally conceded, that none of the local lesions are constant, and so "none can be assumed as the essence of the malady; and, as all may exist, they may each and all be regarded as the results of a cause capable of producing them singly or conjointly." Whether, therefore, the fever takes the form of the "peritoneal," the "gastro-enteric," the "nervous," or the "complicated," where several organs or tissues are attacked at once, a poison in the blood is the one source and cause. So generally accepted is this doctrine advanced by Dr. Ferguson, that Dr. West was in some sort justified in saying, "We have not advanced far, if at all, beyond the conclusions which Dr. Ferguson laid down." In another sense, Dr. West's assertion stops short of expressing the whole truth of the case; for if we have as yet found no better explanation of the phenomena observed in connection with puerperal fever than a "vitiating state of the fluids," nevertheless a whole army of ardent workers have been investigating and adding to our knowledge concerning the nature of the poison which produces such fearful results, the various ways in which it finds its entrance into the blood, and the best methods of obviating its effects. A further impetus has been given to our knowledge of the subject by Virchow, who, after repeating the experiments on purulent infection recorded by Gaspard in 1809, and adopting the term "septicemia" from Piorry as appropriate for forms of blood-vitiation other than by pus, published his researches on thrombosis and embolism. Among obstetricians, the names of Spiegelberg, Schroeder, D'Espine, Barnes, Matthews Duncan, and others, are well known as writers on this subject, who have all contributed something to its further elucidation.

As to the way in which the poison finds its entrance into the systemic circulation, Dr. Ferguson did not overlook the peculiar condition of the interior of the uterus at the placental spot, as affording a ready mode of ingress for what might be deleterious; and he held that all wounds and bruises in the genital canal might create diseased secretions from a traumatic surface, and by absorption give rise to the phenomena of puerperal fever.

Taking, then, the outcome of the discussion in this Society, and the evidence from other sources, it may be stated that puerperal fever, or fevers which are attended by high temperature, and are commonly associated with extensive local lesions, are due to the inception of a morbid poison which vitiates the blood, and which produces a great variety of symptoms in accordance with the nature and intensity of the virus, the amount of the dose absorbed, the state of the patient when attacked, and a diversity of other conditions, which have been alluded to more or less fully by the various speakers.

As to the origin of the poison, whatever may be its nature or chemical composition, Dr. Barnes states that it has two sources : in the one case, the virus is formed in the patient's own body as the result of initial morbid changes in some special part which infects eventually the rest of the system ; in the other case, the virus is imported into the body of the patient by contamination from without. To the one class of cases he applies the term " autogenetic," to the other " heterogenetic "—synonyms which correspond to the appellations " auto-infection " and " hetero-infection " of Schroeder.

In the autogenetic or auto-infective form, septic matter is spontaneously formed within the system of the patient, as the result of disorganisation and death of tissues by a process which Virchow has termed necrobiosis. Thus, the retention of a portion of placenta, or of membranes, or of blood-clots, may become a source of septic poisoning ; or a contusion or laceration of the cervix, vagina, or perineum, may become the seat of unhealthy action, and organic substances in process of decomposition are formed, which, on being absorbed, vitiate the blood. That fluids do pass from the uterine cavity into the uterine sinuses is abundantly proved ; and Dr. Snow Beck has stated, as further evidence on this point, that, after the uterus has been injected by the perchloride of iron to arrest post-partum hemorrhage, the uterine sinuses have been found gorged with grumous fluid containing an abundance of iron. Dr. Savage, who has paid particular attention to the anatomy of the uterus, is inclined to doubt if poison is absorbed by its veins, and thinks the absorbents are the chief agents in this process. Dr. Tilt I understood to hold a like opinion. Dr. Graily Hewitt, Dr. Snow Beck, and others, have enlarged on the great facility afforded to the absorption of septic fluids by an undue relaxation of the uterine walls after delivery, and hence deduce the great importance of thoroughly promoting its contraction upon the expulsion of the placenta. Dr. Hewitt goes so far as to say that he has never seen a case of puerperal pyæmia in which this relaxed condition of the uterus was absent. Dr. Beck attributes the influence which has usually been ascribed to hemorrhage as a predisposing cause of septicæmia, entirely to relaxation of the uterus as the prime factor ; but this, I take it, is a somewhat too extensive statement, as Magendie clearly proved that, with empty vessels, absorption is very rapid in other parts of the body, as well as the uterus. Dr. Tilt attaches great importance to fetid lochia, and their absorption, as factors in producing the febrile affections of the puerperal state. He believes they are not only more frequent than commonly supposed in ordinary cases, but that scarlatina, or other zymotic disease in a puerperal woman, may cause

the lochia to become putrescent and virulent: their absorption then adds a fresh peril to the case, and accounts for the serious character of the complication. And if absorption of decomposing or putrescent materials formed in the patient's own body is to be feared when there is no breach of continuity other than that caused by the separation of the placenta, how much greater may become the danger when any laceration has taken place in the genital canal, which exposes a wounded surface to external agencies! Mr. Callender has reminded us of a truth formerly indicated by Benjamin Gooch, of Norwich, that there is nothing more irritating or acrid than the fluid of a wound itself immediately after operation, and if this be pent up, it may become a source of auto-infection. Mr. Spencer Wells has become thoroughly conversant with this fact in practising ovariectomy, and he drains off from the cavity of the peritoneum the acrid fluid which would otherwise inevitably poison his patient. But Mr. Callender has further informed us that in surgical practice "a patient may be tolerant enough of decomposition which may be set up by a wound in his own body, but will be intolerant of poison conveyed to him or her from any other wound." If this be true in surgery, *à fortiori* it is true in obstetrics, and demonstrates how essential it is to the safety of his patient that the accoucheur should keep his fingers free from all contamination.

Turning to the heterogenetic or hetero-infective forms, in which the virus is imported from without, the inquiry suggests itself, Whence is derived the poison which, introduced into the system, has such potent results? Is it always one and the same, or are there several poisons capable of producing different diseases in non-puerperal patients, but provoking generically the same results when introduced into the body of a woman recently delivered? It is this part of our problem which seems to me most difficult to solve; and yet upon a correct solution of it probably depends the reconciliation of many conflicting views, and certainly the adoption of proper prophylactic measures for preventing the spread of puerperal fever.

If the evidence be true in reference to the exanthemata, it is even more forcible, and borne out by a greater concurrence of opinion concerning the serious peril encountered by the puerperal woman when exposed to the poisons of erysipelas, diphtheria, and typhus. Mr. Wells and others have mentioned instances in which there could be no doubt that puerperal fever was produced by direct or indirect communication with erysipelas; and other Fellows had painful experiences to relate, had time permitted, of puerperal deaths caused unwittingly by going from patients having erysipelas to the lying-in room. The connection between the two has long been noticed; and obser-

vations have accumulated with such force and frequency as to constitute absolute proof of close affinity. Many new-born children have died of erysipelas whose mothers were the subjects of puerperal fever; and instances have not been rare where the medical man attending a puerperal fever patient has from her contracted erysipelas, and died also.

My friend Dr. Fordyce Barker, who contends strenuously for the specific character of puerperal fever, admits in his larger work, and also in a recently published lecture, that the close relation, or what he calls the "solidarity," between puerperal fever and erysipelas is conclusively settled. He considers, however, that, although they are interchangeable, they are distinct diseases. I fear that logically this acknowledgment of interchange of symptoms is somewhat adverse to the theory that identity of cause will necessarily produce identity of result. In recognition of the same natural relation, Virchow speaks of puerperal fever as *malignant puerperal erysipelas*.

There seems to be literal truth in the remark made by Dr. West, that "there is not one single solitary cause to which we can refer the symptoms of puerperal fever: that it occurs now from one cause, now from another." With equal truth, also, it may probably be asserted that there is not one puerperal fever, but several, arising from a diversity of causes. The term puerperal fever has been somewhat loosely but habitually applied to all diseases of lying-in women which were attended by febrile symptoms and tended to a fatal termination. In attempting a more accurate pathology, it is well to bear in mind that fever is only a symptom of disease; and that we have to find out what is the underlying disease, of which the fever is but the indication. Perhaps the best practical classification of puerperal fevers which has been suggested, in accordance with our present knowledge, is: 1. The ephemeral forms which depend on some transient cause; and the graver class, divided into (2) the auto-genetic or auto-infective form, and (3) the heterogenetic or hetero-infective, in which the poison is imported from some zymotic or other infective source, and the origin of which can be traced. Notwithstanding this classification, there will always be cases the origin of which it is difficult or almost impossible to detect. These must necessarily form a class by themselves, until further knowledge enables us to assign to them their proper place.

For a long time, the contagiousness of puerperal fever was disputed, and in Paris writers and practitioners remained unconvinced of its contagiousness long after it had been conceded elsewhere. In our day, the direct proofs of its contagious quality have accumulated to such an extent, that to disregard them in practice would amount, on the part of a medical prac-

itioner, to a plain dereliction of duty. So far as some of the forms are concerned, however the attack may have originated, if it be but a single case, it may become the focus and centre of propagation to other women. And, of the several ways in which the subtle poison may be conveyed to the lying-in woman, none is more certainly proved than that it may be conveyed by the hands of the practitioner in making vaginal examinations. But this is not the only way. It may probably be communicated through the clothes, instruments, respiration, or even the secretions of the medical practitioner, midwife, or nurse. That it may be so spread, we have ample evidence, if we look back to the records of Gordon, Armstrong, Gooch, Ramsbotham, and others. Dr. Roberton, of Manchester, gives one of the strongest instances possible. In the space of one calendar month, a midwife attended twenty cases belonging a lying-in charity; of these, sixteen died of puerperal fever. Other midwives of the same charity attended 380 cases in the same district and at the same time, and none of these were affected by the disease.

There is very cogent collateral evidence towards proof that the blood of a puerperal patient may be infected by other channels than by the genital canal. It is certain that puerperal fever may begin in a patient before her delivery. Dr. Barnes's statement that he has been conscious of absorbing poisonous and offensive emanations from patients with putrid discharges, and that he has suffered from shivering, diarrhoea, and foul breath as the result, affords a very strong presumption that an attendant may go about charged with an infectious disease, and convey an atmosphere about him which shall be most deleterious to susceptible patients. There is a very ingenious exposition in reference to this matter in Dr. Tyler Smith's "Manual on Obstetrics," and he strengthens the position by reference to cases in which pregnant women who have had small-pox in childhood, and are proof against infection, have nevertheless, as in Dr. Corde's case previously mentioned, on being exposed to the poison, conveyed the disease to the fetus *in utero*.

My friend Dr. Moir, a former President of the College of Physicians in Edinburgh, has sent me the following short history, which seems to indicate that a diseased child *in utero* may infect the mother with a form of pyemia which may be transmitted on to other lying-in women. I quote his own words:—

"Mrs. H., who had had several previous very easy confinements, had threatenings of labour on a Monday. Instead of a rapid delivery, as usual, she had slight pains and much malaise all the week, and was confined latish on the Saturday evening. Child above the average in size, about 9 lbs., fat

and healthy-looking. On Sunday forenoon the child died very unexpectedly and without apparent cause. On examination next day, it was found that one pleural cavity was filled with thick purulent matter, the lung being forced back and, of course, undistended with air. The mother never had milk, as usual, fevered with much general pain, and afterwards had all the symptoms of puerperal fever. Large abscesses formed in various parts of the body, some being opened, which gave partial relief. They were on the legs, arms, chest, and a very large one implicating almost the whole dorsal muscle. This one was not opened. The pyemia in this case was evidently existing before delivery from the symptoms, and from the child's being affected. So much for Mrs. H.

"Two hours after her child was born, I was called to Mrs. W., who had had several children: easy cases. She was ill only two hours; took rigors on the third day, and died about six days after delivery, with all the symptoms of puerperal fever. Two hours after Mrs. W. was confined, I was called to Mrs. F., who also had an easy delivery; had all the symptoms of puerperal fever; but recovered after a hair-breadth escape, not recovering for six months. Observe: the three cases were all over within eight hours. I undoubtedly carried the infection from Mrs. H., whom I had been seeing constantly during the six days she was ailing. She survived the delivery about twelve days; Mrs. W. only about six."

But are all forms of puerperal disease attended by febrile disturbance equally infectious? This is an inquiry which bears with moment on the necessity for precautions on the part both of doctors and of nurses, and the question is constantly being raised, Should a nurse who has been attending this or that modification of the disease be permitted to pass on at once to the charge of some other expectant patient? On this point, Dr. Hicks says "that the majority are contagious, I have no doubt; whether all are so, I am uncertain. I am inclined to think that those forms derived from the zymotic diseases are the most so; those from the self-generated kinds are least so." Dr. Routh concludes that some forms are contagious and others are not so; and Mr. Wallace believes that it is "what would be called the pyemic cases which are non-contagious." Dr. Barnes, again, states that "the autogenetic forms proper do not appear to possess active powers of propagation;" and Dr. Fordyce Barker, concurring in this remark, regards this difference of contagious property as one of the proofs that puerperal pyemia or septicemia are generally different from true puerperal fever. I take it, then, that opinions coincide pretty generally that some forms are highly contagious and infectious, others are less so; and it is probable that those arising from

extraneous sources are most contagious; those from poison generated in the patient's own tissues least so; or perhaps I may put it in a somewhat more practical form, as a broad rule for guidance, at the risk of being less scientific and precise, that the most acute forms, running to a rapid termination, are most likely to be highly contagious; those which run a more chronic course, and are attended by secondary complications, are least so. Nevertheless, as we do not know yet that even the chronic cases may not in a minor degree be infectious, it behoves us to exercise caution in attending these cases, to purify nurses who have been in charge, and, above all, to be careful not to convey any of the discharges to other lying-in patients.

The process of parturition should be conducted with the view to the genital canal of the woman being exposed to the effects of irritation, continued pressure, and laceration, as little as may be possible. After the birth of the child, a full and perfect contraction of the uterus should be secured, by seeing that the organ is not emptied too suddenly, and that the hand follows the fundus down from above as its contents are expelled. Subsequently to delivery, the various known methods should be practised to promote the contraction of the walls of the womb and the diminution of the uterine cavity; and any clots forming, should be removed from time to time during the hour after removal of the placenta. It is superfluous to say that the placenta should be extracted in its entirety, when possible; and great care should be taken not to leave any detached portions adherent to the uterine walls. The same precautions should be observed, when practicable, in cases of abortion. If any considerable laceration of the perineum have taken place, the edges of the wound should immediately be united by sutures, not only for the purpose of restoring the perineum, but also to prevent contamination, of the wound by putrid discharges. In the case of other wounds in the vagina or cervix uteri, especial care should be taken to keep them clean by repeated injection, and to leave as small a raw surface exposed as may be possible. In all cases where the lochia are in the least degree offensive, the vagina should be well injected with Condyl's fluid and water, or other innocuous disinfectant, twice in twenty-four hours, or oftener if necessary; and the injection may be carried into the uterine cavity, if it be much distended, and there is a suspicion that it harbours fetid contents. The injection of the womb-cavity, however, should be conducted slowly, carefully, and without force. These uterine injections were practised long ago by William Harvey, and there is concurrent testimony in this and other countries of their marked utility in abating the symptoms even when puerperal fever has apparently set in.

Next in importance is to take care that there is no fecal accumulation in the bowels, and to recollect that the existence of previous diarrhoea may be the indication that retained masses are lying in the intestines and producing irritation there. Dr. W. T. Greene has alluded to this during the debate; and it must be within the experience of many how acute febrile symptoms, accompanied, it may be, by delirium or incipient mania, have passed away with a proper evacuation of the bowels. I may put it, in short, that, to promote recovery after parturition, every direct or reflex source of irritation should be removed, pure air and water insured, absolute cleanliness observed; and perfect quietude, mental and physical, should be enjoined. Nursing the child should also be recommended when the mother's condition permits, as puerperal ailments have been observed more frequent in women who have not suckled their children.

To secure the isolation of a lying-in woman from noxious influences which may be communicated to her from without may be difficult, and in some cases perhaps impossible; but as Mr. Callender has informed us that surgeons have learned to banish, for all practical purposes, those affections which may be termed septicemic from surgical wards of hospitals, so I cannot help thinking that the accoucheur will in time succeed in preventing the like affections in puerperal women, so far, at least, as the heterogenetic cases are concerned. We are all agreed as to the absolute necessity of preventing, directly or indirectly, any communication between cases of erysipelas and puerperal patients. We are agreed also as to the necessity of the midwifery practitioner avoiding all post-mortem examinations. His hands should avoid all contact with specific poisons or septic materials; and if perchance his fingers have touched anything suspicious, he should at once carefully and thoroughly disinfect them. The necropsies which seem most baneful are of those bodies which have recently died of erysipelas, peritonitis, zymotic disease, or any other inflammatory and febrile affections; and in such instances the accoucheur should not even be present in the room when the dissection is made, as, though he decline to touch, yet his person and clothes may become infected by the poison. There may possibly be a doubt whether one who dissects bodies altered by decomposition or antiseptic injections, as they are commonly found in the dissecting-room of medical schools, is so liable to contaminate a puerperal patient as he who makes a necropsy soon after death. Semmelweiss, however, believed that animal matter in a state of putrefaction would cause puerperal fever, and immediately lessened the mortality, during the fearful epidemic of puerperal fever which raged in the Vienna Hospital in 1846 and 1847, by

obliging all dissecting students to wash their hands in chlorine or chloride of lime before making vaginal examinations. Impressed by Semmelweiss's views and opinions, I endeavoured, during my occupancy of the chair of Obstetric Medicine at King's College, to prevent students from frequenting the dissecting-room and post-mortem theatre from attending midwifery cases, and the result was a very small mortality in the outdoor maternity charity.

This care about absolute cleanliness of the hands in attending labours should extend even to occasions when a medical man has had digitally to examine patients suffering from offensive discharges either proceeding from the surface or from the interior of the body. We have heard in this discussion that the discharges from cancer may produce fever in a lying-in woman; and this is probably true of other putrescent discharges. I have had grave anxiety, extending over some weeks, about a patient after her delivery; because I had gone straight from an instrumental case where labour had been so protracted that febrile symptoms had set in, and the passages had become influenced, so as to pour out an irritating muco-purulent discharge.

When a medical man has a bad case of puerperal fever in his own practice, or is required to see one in consultation, he should certainly not go direct to another midwifery patient without first changing his clothes, besides careful ablution of his hands; and he will be all the safer in his ministrations if he adopt some of those precautions mentioned by Dr. Swayne and others, such as using a Turkish bath, or the more radical measures for disinfection recommended by Dr. Wynn Williams, and for the details of which I may refer to his speech.

Many chemical substances have been recommended to be added to water for purifying the hands: iodine, chlorine and its compounds, sulphurous acid, cyanide of potassium, carbolic acid, and the permanganates. It matters not which agent is employed, so long as it is used carefully and efficiently; and if it be necessary to disinfect clothing, this is readily done by exposing it in an oven to a high temperature, for which many upholsterers have a suitable arrangement. With all deference to Dr. Matthews Duncan, I have no doubt that every medical practitioner who earnestly desires to promote the welfare of his patients and to keep himself blameless should at once abstain from attending fresh cases of labour when one or more of those recently delivered by him have died with acute febrile symptoms, or are still so ill as to require his constant visits. When, indeed, there is the remotest suspicion that he may convey the poison in his own person, he should not incur the risk of disseminating it. How long he ought to absent himself from

midwifery practice, is yet a moot point. Dr. Swayne thinks a medical man should seclude himself for a week only; others think a month hardly sufficient. The remarkable experience of Dr. Huntley, of Yarrow, led him to believe that he actually generated poison in his own body, as the result of some faulty process, which he communicated to his patients. Taking all the circumstances into consideration, I am disposed to recommend a week's seclusion after regular attendance has ceased on a single puerperal fever case. When a series of cases have occurred in the practice of any one medical man, he should absent himself from midwifery practice for a month at least. During either the short or the longer interval of seclusion, the means for disinfection should be fully carried out.

Finally, I have to say a word or two as to the propriety of attending patients suffering from scarlet fever or other zymotic disease, and lying-in women, at the same time. After what has been said in this debate, I cannot expect the same accord of opinion as on other parts of the ground I have gone over; but I would earnestly beg those who have as yet had so favourable an experience, when attending the two sets of patients conjointly, to ponder well what has been said by others on the reverse side of the question. It cannot be expected that men in general practice, who may be in charge of a scarlet-fever or small-pox patient, shall at once relinquish all midwifery practice for the time being, because zymotic diseases are so prevalent that this would practically preclude their attending confinements altogether, or make their attendance on midwifery patients so irregular as to be unreliable. Nevertheless, in view of the dangers which have been indicated by various authorities, albeit their experience may seem to point to different conclusions, they are bound to exercise vigilance, lest perchance they slip into a pitfall unsuspected by them. In seeing an ordinary case of illness, a medical man rarely stays sufficiently long in the sick-room to concentrate any considerable dose of contagium about him, and the after-exposure to fresh air in passing from house to house no doubt usefully dissipates any smaller quantity. I would suggest, however, to those treating infectious cases, and liable at the same time to be summoned to cases of midwifery, or who may be in attendance on puerperal women, that they should not stay long in the sick-chamber; not undertake the duties of nurses, as Dr. Duncan puts it; that they should see puerperal patients before seeing infectious cases, and never go from the infectious case direct to the lying-in room, without changing clothes and the most careful ablutions. Some doctors change their clothes and wash with a disinfectant whenever they have seen infectious cases. Whatever either will, or may, conduce to the well-being of patients, will, I am sure, not be

regarded as either too irksome or troublesome by any member of our profession.—*Obstetrical Journal*, Feb. 1876, p. 741.

93.—ON THE USE OF THE FORCEPS AT THE ROTUNDO LYING-IN-HOSPITAL, DUBLIN, DURING 1875.

By Dr. GEORGE JOHNSTON, Ex-Master of the Hospital.

Alluding to the question of the use of the forceps, Dr. Johnston said: "There were 113 cases where we considered it advisable to deliver with the forceps, and 83 of these were primiparæ, or more than two-thirds, giving birth to 53 boys, 51 of whom lived; two were dead at birth; 30 were girls, 27 of whom lived; 1 was dead at birth. 75 mothers recovered; 8 died, 6 being cases of seduction, fretting; 2 cases of peritonitis; 30 were pluriparæ, in which 16 boys were delivered, 14 of whom lived; two were dead born; and 14 girls, all of whom lived. 28 mothers recovered, 2 died, 1 of pleuritis, 1 of fungoid tumour of the uterus, with fatty heart; making a total of 69 male children born, 65 being alive at birth, 4 of whom died; 4 were dead born. Thus, 61 males were saved, and 44 female children born; 43 were alive at birth, 2 of whom died, and 1 was dead born. Thus, 41 female children were saved, making a total of 102 children saved who, in all probability, would have been lost if left to the natural efforts; 103 mothers recovered, and 10 died. The forceps which we at first used were of the straight form, both long and short, generally called Beatty's; but finding on frequent occasions that they slipped, particularly when the head was above the brim, or when it was at all tightly wedged high up in the pelvis, and were consequently inefficient, we were obliged to have recourse, about five years since, to the double-curved variety of Barnes, and we have great satisfaction in saying that the more we employ them the more reason we have to be perfectly satisfied with them, inasmuch as we have proved them to be an instrument both safe, easy in their application, and most efficient in their powers of extraction, and at the same time not compressing the child's head to any injurious extent, and perfectly available, whether the head may be above the brim, in the brim, or in the cavity; in fact, we see no necessity for using any other, as they can be introduced by one experienced in their application as easily, if not more so, than the straight variety in all cases; and, as a proof of their not producing any injurious effect on the child, instances are given in our former reports where it has been born alive and lived, although the force used to draw the head through the contracted strait was enormous, and the length of time occupied by the operation was very great. One case may be mentioned as serving for an example, which occurred in a woman in her ninth

pregnancy, where there was an exostosis of the right sacro-iliac synchondrosis, preventing the head from entering the brim, and where it took three-quarters of an hour to extract the child, whose head, when born, presented a depression occupying the prominence of the left parietal bone of over 3 inches long and $1\frac{3}{4}$ inches broad, and at least $\frac{1}{4}$ inch deep. Yet that child, a boy, weighing 7 lbs. 12 oz., lived, and was carried out by its mother quite lively and well on her being discharged on the eighth day. Now, if we deduct the 40 abortions from the total number of deliveries recorded as having taken place in the year, it will leave 1,025, which, divided by 113, the number of times the forceps were used in that period, it will be seen that they were had recourse to once in about every 9 1-11th. Some may cavil at the system we have adopted, but the further we pursue the practice the more we see the advantages arising from it in preventing serious consequences to both mother and child—the mother being saved from lengthened agony, prolonged too often to a serious extent from the effects of exhaustion, rendering her liable to inflammation of the soft parts, with all its direful results, such as sloughing and its consequences, fistula, pyæmia, peritonitis, &c., and the child's life preserved by not allowing it to encounter too long the danger resulting from the protracted pressure, both by the compressing of the head in the pelvis and the evil results of the contraction of the uterus round the child and the placenta, thus cutting off the supply so essential to its foetal existence. In reply to the objection that has been raised by some—viz., that the forceps is a dangerous instrument, and as a proof to the contrary, I may mention that of the 752 cases that have been delivered within the last seven years, in no one instance was injury inflicted by the instrument on the soft parts of the mother. In fact, by using proper precaution in their application, by bearing in mind the different axes of the pelvis and the condition of the passage and cervix uteri, and by introducing the blades with the greatest gentleness in accordance with the direction of these axes, and by taking every care to avoid catching the points of the blades in the cervix, particularly when it is not quite fully obliterated, and when they are applied by making the traction in the proper direction, *i.e.*, in accordance with the amount of descent at which the head may have been arrested in the pelvis, and by continuing it in the axis of the pelvis your extractive force has brought it to, which latter precaution should be particularly attended to, and as soon as the head has been brought sufficiently forward to the outlet, by withdrawing the blades, as our invariable practice is, and has always been, and completing the delivery in the ordinary way, there cannot be, in the hands of the skilful practitioner, any injury produced. And no doubt whatever ex-

ists that such practice tends to the more favourable convalescence of the patient. Now, although far from encouraging or inculcating "meddlesome midwifery," as it has been termed, and no doubt in some instances not without reason, as this practice should never be adopted merely in order to save our own time, it should alone be had recourse to when the life of the mother or that of her offspring is in jeopardy. The more we see of early interference and the benefits arising from it, the more we are induced to persevere in it. It has been insinuated that it is a most dangerous practice, and therefore it would be wrong to inculcate it into the mind of the general practitioner; but although no doubt it would be so in the hands of a careless or unskilful person, should that be a reason, may I ask, why the skilful obstetrician should not adopt a measure attended in his hands with perfect safety, by which the many hours of suffering agony of the mother is spared and the life of her offspring preserved, which, under the peculiar circumstances of the case, would otherwise be endangered, if not altogether lost? It might as well be said you should not attempt to take up the subclavian, tie the external iliac, perform lithotomy, or any other of the operations in surgery, because, forsooth! such would be dangerous or unjustifiable in the hands of the unskilful. We have only to add that it has been, so far as our experience goes, the means of saving many lives, both of mother and child, and therefore we have every reason to consider ourselves not alone justified in the continuance of it, but in also recommending its practice to others. At the same time, while doing so, we warn all, as we have always done the student in the class-room, of the dangers to be avoided in using the instruments, not only in the first stage of labour, but in every case, where, even although the os may be fully dilated, the absolute necessity there exists of using the greatest caution and gentleness in the introduction of the blades, in their application, and when adjusted, in the mode of extraction.—*Medical Press and Circular*, Jan. 19, 1876, p. 43.

94.—CASE OF ARTIFICIAL OCCLUSION OF THE VAGINA FOR THE CURE OF PROLAPSUS UTERI.

By Dr. JAMES DUNLOP, Surgeon to the Glasgow Royal Infirmary.

Margaret F., aged forty-eight years, was admitted February 20, 1875, suffering from prolapsus uteri of fifteen years' standing, and attended with the usual symptoms—viz., a sense of fulness in the pelvis, pain in the back, with some irritation of the bladder and rectum. The uterus was resting on the upper floor of the perineum, and protruding somewhat through the

vulvæ. It could be easily replaced on the patient assuming a recumbent position. Various kinds of pessaries had been employed, but the patient was unable to wear any for a length of time. Other means of support had also been had recourse to, but without avail. An operation was therefore proposed, to relieve the patient of the discomfort with which such a malady is necessarily attended. Its object was to narrow the vaginal orifice by uniting the opposite portions of its walls, so as to bring forward the perineum, as it were, to act as a natural cushion to the prolapsed parts. A strip of mucous membrane an inch and a half wide was dissected off from half an inch below the meatus on one side to a corresponding point on the other. The two raw surfaces were now brought into close apposition, and kept there by means of quilled sutures. The catheter was passed frequently, and the bowels were kept confined by the administration of opium. The parts speedily united, and the patient was dismissed well on April 30, 1875.—*Medical Times and Gazette*, Feb. 26, 1876, p. 223.

95.—A NEW METHOD OF PREVENTING THE SECRETION OF MILK IN THE FEMALE BREAST.

By Dr. JOHN WILLIAM LANE.

I have for more than ten years employed the following method to prevent the secretion of milk in the breasts of women who may have had still-born children, or who, after having nursed their child for a few months, found it necessary to wean it. It is perfectly clean and painless as far as my experience goes, and as such I beg to recommend it to the notice of my medical brethren.

We will take for instance the case where the infant has been born at the full period, but is dead, or dies within a few hours of its birth. The milk makes its appearance in the breasts generally about the second day, sometimes longer, and sometimes it is ready when the child is born, and in the case of still-born children my experience leads me to think that in such cases it makes its appearance earlier than when the child is born alive. My plan consists in taking a piece of emplastrum adhæsivum of about ten inches square, round the corners, cut a hole in the centre for the nipple, then from the centre of each corner make a straight cut towards and within two inches of the centre hole; having now got it ready, let the patient lie on her back, her body being perfectly horizontal; warm the plaster and place it over the breast, then strap one of the lower corners down first, draw the opposite one tightly upwards and fix it in its place, then the other lower corner, and lastly the opposite upper one, having drawn it sufficiently tight

first ; now take a piece of plaster two inches wide and about sixteen or eighteen inches long and put it on from below and outside the breast, across close by inside of nipple, and fasten the end over the clavicle ; another piece may also be put on in an opposite direction, it being drawn over the shoulder. Of course, in cutting the plaster and strips the size of the breasts must be taken into consideration, there being so much difference in the size of female breasts.

The above plan I always follow when one of my patients wish to dry the milk, as they usually call it, or where they are compelled to do so either from the death of the child or any other cause. I also am certain strapping will prevent mammary abscess if resorted to in the earlier stage ; I at least have found it do so in many cases.—*Medical Press and Circular*, Feb. 16, 1876, p. 131.

96.—“NO MORE OVARIOTOMY.”

Under the above startling title we find a note in the *Surgical Centralblatt* for Feb. 12, taken from the *Wiener Med. Presse*, 1875, No 52, by Dr. Semeleder. About two years ago he was informed that a lady of his acquaintance suffering from an ovarian cyst, who had been much relieved in Dresden by acupuncture (? galvanopuncture), had been ultimately cured in Vienna by the same treatment. Since that time he has tried it in three cases :—

1. A young lady, aged eighteen, who had a soft fluctuating ovarian tumour, originating on the left side and extending three centimetres above the umbilicus, was subjected to galvanopuncture. In four months the diameter of the abdomen two inches below the umbilicus was reduced from ninety-six centimetres to ninety-two centimetres ; and in two months more the cure was completed.

2. A lady twenty-four years old, and the mother of two children, had a tumour in the lower part of the abdomen on the left side, as large as the head of a child two years of age. When she had been under treatment for two months the patient was cured, the remains of the cyst being hard, and of the size of a small apple.

3. A woman forty years of age, with a tumour reaching up to the umbilicus, had so far recovered at the end of six weeks of the treatment that its continuance was considered unnecessary.

No unpleasant consequences occurred in any of these cases, and none of the cysts have refilled. The author considers that the action is the same as that which occurs when the poles of a battery are placed in an albuminous fluid—viz., clotting and

thickening at the positive pole, and liquefaction at the negative. He considers the method equally applicable to multilocular and unilocular cysts. He does not give an exact account of his method of procedure, but each sitting was of short duration. He anticipates equally favourable results in the treatment of hydatid cysts on this plan.—*Med. Times and Gazette*, March 4, 1876, p. 256.

97.—ON THE USE OF NITRIC ACID AS A CAUSTIC IN
UTERINE PRACTICE, AND ITS SUPERIORITY
AS SUCH TO NITRATE OF SILVER.

By Dr. JAS. BRAITHWAITE, Lecturer on Diseases of Women and Children at the Leeds School of Medicine; Assistant-Surgeon to the Leeds Hospital for Women and Children.

I believe that, in cases in which the use of a caustic is necessary for the cure of ulceration or erosion of the os and cervix uteri, that which is generally, but not invariably, the most suitable for our purpose is nitric acid.

I do not propose to enter upon the disputed question of the pathological importance of these affections, nor upon any other point in their treatment, such as the primary relief of congestion or inflammation. I cannot, however, with Dr. Routh (*Transactions of Obstetrical Society of London*, vol. xi., page 94), look upon the use of caustics as solely for the purpose of relieving congestion by means of the discharge produced, in which case nitric acid would be useless, for it produces no discharge. I find that by curing the ulceration we cure the congestion, generally, but not always, without other treatment.

I think it will be admitted that nitrate of silver is the caustic in most general use, and that it is unnecessary I should take up your time by references to writers on the subject to prove this.

The great fault of nitrate of silver for the purpose in question is the fugitive nature of its action. Its influence seldom extends beyond five or six days, even when rubbed upon and held in contact with the parts. Again, nitrate of silver is more a stimulant than a caustic, causing extreme turgescence of the capillaries immediately below the surface actually influenced. This is evidenced by the frequency of hemorrhage, often sufficient to obscure the parts before it has been removed from contact with them. The escharotic action of nitrate of silver, such as it is, is too superficial to destroy the diseased surface, and by the production of a slough to stimulate the parts beneath to the healthy activity necessary for its separation. At the second examination, we often find the ulceration or erosion, very little, if at all, altered in appearance.

The action of nitrate of silver is thus defective for our purpose—1, in duration; 2, in quality; and 3, in degree; and these faults must be atoned for by its frequent reapplication. This necessitates each time the use of the speculum, distasteful alike to patient and to attendant. I believe in this really lies the source of the opinion held by some eminent men that these diseases require little or no local treatment. Our opinions are often unconsciously influenced by our wishes. Dissatisfaction with nitrate of silver has caused the trial of other applications such as sulphate of zinc points, nitrate of mercury, potassa cum calce, ignited charcoal-points, and electric heat, all of which either have some disadvantage or are ineffectual. I believe nitric acid to possess these advantages as a caustic in uterine practice. It requires no special preparation, and is always at hand; it cannot affect the system as may a salt of mercury; it does not spread like potassa cum calce, nor is its action so deep; it produces little or no pain, and no hemorrhage. These advantages are trivial compared with the fact that, when it has once been properly applied, in the majority of cases no further interference is necessary, and thus that opprobrium of our branch of the profession—the frequent use of the speculum—may be done away with. When the second examination is made, it should be after the lapse of a month; and it will then sometimes be found that there is a small spot requiring a fresh application of the acid, but often the sore is seen to be quite healed, or healing satisfactorily. The eschar separates slowly, and the resulting sore has a very strong tendency to heal. The fresh mucous membrane which forms is not cicatricial in appearance, and, when healing is going on satisfactorily, has a sharply-defined edge, and, being of a pale rose colour, contrasts strongly with the bright red colour of the sore. The contraction is greater than follows the use of any other caustic; but this is a great advantage, for, on account of the relaxed state of the tissues, it is just what is required to insure the permanence of the cure. The acid is best applied by means of a small and tightly-rolled piece of cotton-wool, which is to be placed by means of an ordinary speculum-forceps in contact with successive portions of the surface, until the whole is covered with a white eschar. In a case of chronic endocervicitis, the acid should be applied to the interior of the open cervical canal; and, if it be not open, the case is not one suitable for this treatment. The contraction which accompanies healing is only to a healthy and natural state, provided the caustic has been used with ordinary prudence. I have never seen anything but good follow its use, and the ease with which a chronic case of cervical catarrh with ulceration or erosion may be cured by it is something marvellous. The bulk of my cases

have been hospital out-patients, and the comfort the use of nitric acid has been in their treatment is very great, both in certainty of result and in saving of time. Without local treatment, very little can be done for these patients, for hygienic treatment is generally impossible, and medicinal treatment alone is useless. I shall not take up your time by details of cases, although I have copious notes of about forty, as by trial of the remedy you will soon prove your own opinion of its value.

The use of nitric acid as a caustic is so familiar to us all, especially in diseases of the rectum, that I have had some hesitation in bringing the subject before you, and should not have done so, but that I believe it is only used by two or three medical men engaged in the treatment of diseases of women. It is mentioned incidentally at the conclusion of a paper by Dr. Lombe Atthill, upon its application to the interior of the uterine cavity, that he uses it habitually in the diseases in question; and Dr. Roe, of Dublin, in an analysis of the cases of uterine disease (*Dublin Journal of Medical Science*, August, 1872), relates a case of extensive ulceration in which he employed it. Mr. Robert Ellis has recommended the use of a saturated solution of nitrate of silver in nitric acid; and I believe Dr. Bennet has mentioned its use. But these writers are exceptions to the general rule. It is not mentioned by Tilt in his admirable work on *Uterine Therapeutics*, nor by any other of our standard authors upon diseases of women, all of whom recommend nitrate of silver, or mention its use as the usual practice.—*British Medical Journal*, Nov. 13, 1875, p. 609.

98.—ON MAMMARY TUMOURS.

By Dr. GEORGE BUCHANAN, M.A., Professor of Clinical Surgery in the University of Glasgow.

[After some preliminary remarks Dr. Buchanan relates the following case.]

This woman is forty years of age, unmarried, of spare habit of body; and hitherto she has enjoyed good health. The catamenial function was regular from its appearance up till eighteen months ago, since which time it has altogether ceased, without any cause. Three months ago, she became aware of a tumour in her right mamma. Her attention was drawn to it by feeling her dress a little tight. There was no pain when she discovered it. During the last three months, it has increased in size, and the growth has been more rapid of late, especially during the last two weeks, following on a slight blow on the back of a chair; though, remember, I do not say that was the cause. In

the right mamma, occupying the upper portion, there is a tumour which is irregular, nodular, about four inches in diameter, and to the feel about three inches thick. The tumour and the gland in which it is imbedded move freely upon the subjacent structures, and are not in any way adherent. Immediately above the nipple, the skin is partially adherent to the tumour; but towards the circumference it is freely movable upon it. The nipple is slightly retracted, but not to any extreme degree. The axillary glands are perfectly free from implication. So far as the patient's sensation is concerned, there is no difference between the two mammæ, except what is produced by the weight of the tumour on the right side. I may say negatively that there is no shooting or burning pain. The tumour is, as I have said, slightly irregular in form, and hard to the feel.

Such is the case, and a very valuable case it is to learn from; because, although I have no doubt in my own mind as to the nature of the tumour, if you take any work upon the subject, and examine into the symptoms there given of scirrhus mamma, you will find that, in a great number of important points, the symptoms are here absent; yet there is not the slightest doubt that this is a case of scirrhus mamma; and that, I hold, is the value of clinical observation as compared with doctrinal teaching. If a surgeon go and examine a case of this kind with his mind filled with the tabular statement of symptoms, the absence of many of the most important of these symptoms might lead him to believe that it was not a case of scirrhus. But by bringing the patient before you, and making her tell, in her own simple way, all that she knows and all that she could tell you in private, and by examining the tumour simply and regularly before you, and by learning everything about it, we are driven to the conclusion that in practice we must meet with a great variation from the ordinary run of cases as described in books.

What is the nature of this tumour, and what should be done in the circumstances? Now, I do not intend to give you a list of all possible tumours, or of all growths in the mamma or other parts of the body; but I think it is very important to put before you a clinical view of the subject. Tumours of the mamma are of two great classes—the malignant and the non-malignant. Simple or non-malignant tumour consists of the collection into one spot of one of the normal constituents of the body. Thus, confining our attention to the mamma, we may have fatty tumour, fibrous tumour, adenoid tumour; and these are simple, because they consist of the same substances which form normal parts of the body. Malignant tumour, on the other hand, is formed of a collection of substances which are a

new growth in the human body ; of abnormal substances which do not exist in the body of the living man. That is the essence of the distinction between the two kinds of tumour. If you examine the minute structure of the simple non-malignant tumour, you will find it to consist of a modification, it may be of one of the normal tissues of the body ; if you examine the malignant tumour, you will find it to consist of tissue of which there is no example in the human body.

With regard to the mode of growth, simple tumour extends by increase of bulk, displacing, pushing aside, perhaps causing to be absorbed, the tissues with which it is surrounded. Malignant tumour increases in bulk by incorporating with itself the adjacent tissues, and gradually converting them into itself. Further, simple tumour is for the most part isolated and incapsulated, contained in a membrane which is called its capsule. Real malignant tumour has no absolute defined membrane, but invades all the neighbouring tissue without any well marked margin. The ordinary name of cancer is well given to malignant tumour, because it gradually draws into its centre all the neighbouring parts, so that in the end it extends without limitation.

Now it is very important for you to mark well a few of these characteristics, because upon them will depend the opinion that you will give with regard to the propriety or impropriety of operating upon a patient. It is perhaps unnecessary to go further just now ; but it will complete the clinical view of the case if I state that, while it is true that there are two great classes divided from one another by well defined marks, simple and malignant, there is a debatable ground between the two—a variety of tumours or growths which it is exceedingly difficult, if not impossible, to assign either to the one or to the other ; and, in addition, the malignancy of the tumour occurs in different degrees of intensity. That form of malignant disease which is called encephaloid, that called scirrhus, and that called colloid, are three varieties in which there is no doubt whatever of the intense malignancy—I mean of their inherent abnormality of growth, if I may use the term, and their enormous tendency to return if removed ; so much so that, in dealing with tumours of that kind, we are absolutely unable to say that we expect them not to return, though the time of their return is a matter of doubt. But then there is another kind of malignant tumour, which, though properly called cancer, is less malignant than the others, and this less malignancy is shown both clinically and microscopically. Epithelioma or epithelial cancer differs from those which I have previously mentioned in being an exaggeration of the epithelial tissue. I beg you here to remember that I am not going into the deep pathology of tumour ; because,

were we to go a little farther than we are doing at present, we would fall into the discussion between the epithelial and fibrous pathologists. There is one class of pathologists who hold that all tumours, especially malignant tumours, arise altogether from an abnormal change taking place in the epithelial tissues. Another set hold that all malignant tumours arise from an alteration of the fibrous structure in the particular part. Epithelial tumour resembles closely the natural epithelium only it is more complex in its arrangement. In this way, therefore, while in internal structure it has a certain resemblance to the true malignant tumour, it partakes, also, of the character of simple tumour clinically, because it can be removed with success.

But there is a kind of tumour which belongs to the simple or non-malignant fibrous kind, which partakes of malignancy, inasmuch as it returns after removal. Such is the tumour which used to be called recurring fibrous tumour. You will observe that I am adhering to the old nomenclature; and I do so with the view of conveying information to the comparatively junior students. When you enter the pathological class, you will find that tumours are differently classified.

Having, then, stated these elementary views as to the character of tumours, let us now say a word or two with regard to their treatment. The question I am going to discuss is not so much the possibility of treating these tumours medically in the way of palliation, but particularly with regard to surgical removal. I wish, however, to tell you at the outset that, because a person has a well defined tumour in the mamma, it is not absolutely necessary to excise it. I shall say nothing at present with regard to removal by caustics; but it has fallen within my own experience to have seen several most remarkable examples of the disappearance, I might almost use the term cure, of tumour by pressure; and that information is, I think, of great value, because in many cases, from the constitution or the age of the patient, or from the implication of the neighbouring parts, you could not, with any degree of conscientiousness, recommend removal of the tumour; but I could show you ladies in Glasgow at the present day who have had tumours in their mammæ, and who are now absolutely free from the disease by the application of careful and well directed pressure. You are aware that pressure will cause absorption both of normal and of abnormal tissue; and you are probably aware that, if a person have an aneurism of the aorta, and if the aneurism continue to grow, it not unfrequently happens, through the tumour pressing upon the sternum, that it gradually induces absorption of the bone until it appears underneath the skin, and, if not arrested, it spontaneously bursts, and causes loss of life. We are all

aware of the importance of pressure in assisting the absorption of abnormal fluids; as by the use of a splint and bandage in cases of effusion into joints. In the same way, pressure, well directed to the breast, has a remarkable effect in causing the absorption of tumours; and I now am in the habit of ordering the application of a properly prepared apparatus in cases where, either from the situation, or the implication of the neighbouring parts, I consider that the operation of excising the mamma would be unadvisable.

But, next, with regard to operation: what is the difference in the probable result or prognosis between excision of simple tumour and excision of malignant tumour? If we excise simple tumour, we remove the whole disease, and there is no more tendency to its reappearance. We can, therefore, undertake its excision, with absolute certainty of success, so far as return of the disease is concerned. We can never, however, promise to perform any operation with absolute freedom from danger, because, in certain states of the constitution, the smallest scratch may end in death. Persons frequently come to the surgeon suffering from a serious disease resulting from very simple causes. We have at present a man in the infirmary with his hand and arm inflamed to such an extent that he is likely to lose them, and all from a scratch at the point of one of his fingers. It does not matter, then, whether the scratch is given by the surgeon or by the patient; it depends upon the state of the constitution whether or not inflammation will ensue. In the excision of simple tumour, then, all that is necessary is that the whole tumour shall be removed—you must not leave a bit of it. But you can do this with absolute certainty; because all that you have to do is to cut through the surrounding tissues, and remove it entire with its capsule. Should it chance to be irregular, and should you leave any part of it, you have simply to take the precaution of dissecting the bit that remains; and, having done this, you may give a certain opinion that there will be no return of the disease. But, with regard to malignant tumour, it is different, inasmuch as it is not isolated or encapsulated, but is of the nature of invading the neighbouring tissues. It is a difficult point to decide how far it extends in the neighbouring tissues; but, taking it for granted that what appears to you to be the limit of the tumour is not the limit, the golden rule is to make your incision clear of the tumour, to cut well beyond it into the sound tissue.

In excising a tumour from the mamma, do not, if possible, leave a single gland-cell—take the whole mamma away. In certain instances, especially in cases of epithelioma, such as may be situated on the back of the hand, on the sole of the foot, or on any part of the limbs, upon the edge of the lip,

upon the face, or upon the extremity of the penis, where epithelial tumours are found, in such situations you can with safety cut away a large portion of the sound tissue beyond the disease; and in these circumstances, you may with certainty predict that the cure will be complete. But, in regard to other tumours not epithelial, there is this one other thing to be said: that, while you can remove a tumour and its surroundings perfectly accurately, the constitutional tendency that has been the cause of the malignancy—of which I have said nothing at present—that tendency still remains; and you can only hope that it will not show itself, and return in some other part of the body. In the case of malignant tumours, all the other parts in the neighbourhood of the mamma are apt to become secondarily implicated.

Most of you are aware, of course, of the absorbents of the mamma. And here I would say to the students who are not yet out of their anatomical studies, that the study of the absorbents in certain regions of the body is very important—as, for instance, the groin, axilla, &c. Now, it not unfrequently happens that a tumour in the mamma irritates the absorbents, that irritation is communicated to the absorbent glands, and they become enlarged. Besides, while the absorbents are naturally adapted for the purpose of carrying away the fluids of the body which are to be used again, in the case of the diseased tissues they take up the diseased substance, and carry it backward to the lymphatic glands, for the purpose of undergoing elaboration. In this way, there is a danger of the lymphatic glands becoming infiltrated with the cancerous matter. It is not because of the proximity, but because of the course of the absorbents from the mamma through the axilla, that the removal of the mammary gland will not cure or destroy the disease; it will be left in the absorbents. Hence, with regard to the return of the disease, it is of absolute importance, so far as the prognosis is concerned, that you should know whether the axillary glands are implicated or not. If they be, you may be certain that the day is not far distant after the operation—it may be weeks, months, or years—but the day will come when the disease will show itself in the axilla or neighbourhood. You may relieve the patient for a long time; but I believe, if the absorbent glands be in any degree affected, it is a question of time.

Such being a general view of the nature of an operation and its results, let us come back to our case. Here is a woman with a tumour of the mamma that presents many of the appearances of scirrhus, yet some of the symptoms are absent. There is, first, the absence of pain. You will find in most of the treatises on the subject that pain is considered diagnostic

of scirrhus—shooting pain, tearing across the chest, very frequently shooting down the arm and giving rise to want of rest. When that is present, it is almost always a diagnostic sign of scirrhus. But its absence is not a proof that it is not scirrhus; for here is a patient who has a tumour as manifestly scirrhus as it can possibly be, as you shall see next Wednesday when we cut it out. Secondly, there is often implication of the neighbouring glands, and that is absent in this case. And there is another point in which this tumour differs from most mammary scirrhus tumours, namely absence of retraction of the nipple. This, which is commonly regarded as one of the symptoms of scirrhus, is simply a mechanical result. In scirrhus of the mamma, the whole substance involved is converted into a firm, hard, rigid structure. The progress of scirrhus is to extend, to draw together what is a soft, irregular, moist substance into a hard, rigid substance, the section of which, in a freshly excised tumour, has very much the appearance of a frosted turnip. The skin of the mamma is more or less movable on it; but the nipple is joined to the gland by ducts and fibrous structures, and consequently, when contraction of the scirrhus takes place, it may recede a little from the skin, and yet the skin does not follow it till adhesion actually takes place; but, the nipple being attached to the gland in which the malignant substance is infiltrated, retraction is simply a mechanical result. In this case, the nipple is partially retracted, but not to such a degree as would indicate scirrhus if there were no other symptoms.

What, then, is the characteristic mark which enables me to say that it is scirrhus? The name indicates. Scirrhus is a substance hard, unyielding, slightly nodular. When these symptoms are present, there is almost no other tumour of the mamma that it can be.

If this, then, be a case of malignant tumour, of undoubted scirrhus, and if it be true that scirrhus, after being removed, has a great tendency to return, either in the former seat of the disease or neighbouring parts, why do we perform the operation? That is one of the most difficult points for the surgeon to make up his mind upon, and it is a very poor subterfuge for him to tell the patient the *pros and cons*, and leave her to decide. In the present instance, the patient is so absolutely unable to come to anything like a correct judgment, that the surgeon should be definite in his opinion. There are a great number of operations in which the patients can assist the surgeon; in which they can make up their minds for themselves. In cases where it is not a fatal malady, in cases where it is a mere deformity of appearance, in these and numerous similar cases, the patient can be taken into the surgeon's confidence. But I do not think it is possible for any patient or friend of a patient in

a case such as the present to put himself into the position of having proper information with regard to it, and it is, therefore, for the surgeon to make up his mind. In not a few cases of patients who have been brought before me in private for consultation, I have said, "No; I shall not operate." In a notable example, I have said, "This operation ought to be done;" but it is very difficult to convey to you the reasons that should induce the surgeon to operate, or refuse to do so.

But let us take the present case as an example. What are the circumstances which should induce me to perform an operation upon this patient? First, as regards the tumour; and, second, as regards the woman.

As regards the tumour, it is perfectly movable upon the subjacent parts; it is quite clearly non-adherent upon these parts; secondly, the skin upon the circumference of the tumour is absolutely healthy; thirdly, the axilla is perfectly unimplicated. These are the three points with regard to the tumour which make it certain that, if operation be properly performed, the tumour and its connections can be removed without leaving a trace of the disease behind it. Now, scirrhus is one of those forms of disease so connected with the constitution, that it is not at all unlikely that, at no distant day perhaps, the disease may reappear in the other mamma, in the axillary gland, or in some other part of your body; but, even supposing I were perfectly certain that within a year the disease were again to show itself, should I not take that into account?

Let us next, therefore, consider the woman. And I think it is a very important thing for students to get into the habit of considering their patients. I am afraid that the tendency both of the surgeon and the students of surgery is to look at the physical condition of the part, forgetting a little the circumstances of the patient. I hope that is not the case with you; but it is important that it should every now and then be brought before you. Here is a woman who has comparatively suddenly become aware that she has cancer in the breast. I did not need to tell her that; she knew well enough, and that was why she came to me. She said, "I have got cancer in the breast." As you saw, she is of a nervous temperament, and she has a very disagreeable and painful feeling with regard to this scirrhus in the mamma. She has the horror of cancer upon her. Unlike many other diseases, cancer creates in the patient who is conscious that he or she has it a feeling of great depression. This patient wonders what will happen. We do not tell her what will happen, but we all know very well. A growth discovered three months ago suddenly puts on activity, which causes it to increase in size very much more than before. Within that time, probably, adhesion to the skin near the nipple has

taken place, and that is the only part that is adherent. What will be the history of the case if I do not touch it? The adhesion to the skin will increase; a little red spot will show itself where the adhesion to the skin exists; within a short time, ulceration will take place, creating a loathsome discharge; the life of the patient will extend to two or three months, during which her whole existence, till death relieves her, will be most painful. That would probably be the history of the case. Well, if we be able to perform the operation in such a way as to enable her to recover from its effects, and if we be able to eradicate the disease, temporarily even, we will then have it in our power to say to the woman, You are free from cancer; your tumour is away; your disease is removed; it is in the hands of Providence when it may return; in the meantime you are a healthy woman. Is it not worth while undergoing an operation which is rarely fatal, rarely dangerous, for the sake of being free from the condition I have described, restored for weeks, months, perhaps years, to a state of perfect health? I maintain that excision of the mamma ought to be performed; and for these reasons, which you may extend to others, I intend to operate upon that woman. I hope and believe the operation will be successful, and, when the wound is cicatrised, she will leave the hospital perfectly healthy for the time being. To show that this is no chimerical notion, I ask you to remember the operation you saw performed the first operation-day in this hospital, where you saw removed from a woman a large tumour somewhat of the same kind. She is not here now, because, two days ago, I gave her an admission here to the Convalescent Home at Dunoon, where she is at present enjoying herself; whereas, if the operation had not been performed, she would have been lying in bed, suffering from a loathsome discharge from a disease which would result in her death in a very short time; and I think that is a great deal gained.—*British Medical Journal*, Feb. 26, 1876, p. 247.

ADDENDA.

99.—AN OFFICINAL MULTIPLE ANTIDOTE.

(From Dr. RICHARDSON's Report on Toxicology in the Medico-Chirurgical Review.)

Dr. DU VIVIER proposes an officinal multiple antidote for poisons. His proposition, which was read by Dr. Jeannel before the Medico-legal Society of France, on the 8th of Feb., 1875, supplies the following as the formula of his antidote:—

Solution of sulphate of iron (sp. gr. 1.45).	100 parts.
Water	800 „
Calcined magnesia	88 „
Purified animal charcoal	40 „

The iron solution is to be kept separately; the magnesia and animal charcoal in a bottle with water. When required for use the iron solution should be poured into the bottle, and the whole shaken well together.

1. This mixture should be administered in consecutive doses of from 50 to 100 grammes.

2. The experience of the author has proved that this antidote renders preparations of arsenic, of zinc, and of digitalis, completely insoluble; that it does not render oxide of copper entirely insoluble; that it leaves in solution a notable quantity of oxide of mercury, and an appreciable quantity of morphine and of strychnine; that it does not decompose nor precipitate the cyanide of mercury; that it thoroughly saturates free iodine; that it acts partially only on solutions of alkaline hypochlorite.

3. The author's experiments upon living animals justify him in arriving at the following conclusions:—That the proposed antidote is perfectly efficacious in the case of arsenical preparations in the proportion of 120 grammes of antidote to five decigrammes of arseniate of soda. It retards the poisonous effects of the sulphate of strychnia, and would perhaps give time for the administration of salutary evacuants. It has been seen to act efficaciously against digitalis injected into the intestines in doses of one decigramme.

4. This formula is certainly preferable to the officinal hydrate peroxide of iron, as this undergoes, by the action of time and of a temperature higher than 15° C., a molecular modification,

which renders it inefficacious against arsenical preparations. This formula, containing peroxide of iron, extemporarily prepared, of magnesia hydrate, and of animal carbon, is a satisfactory antidote in a great number of demonstrations. It is, however, not efficacious against alkaline minerals, phosphorus, the hypochlorites, the cyanides, and tartar emetic.

5. The protosulphate of iron, dissolved and mixed with magnesia and sulphate of soda, is of absolute chemical efficacy as regards the salts of copper. As regards bichloride and cyanide of mercury it seems preferable to the extemporised hydrated peroxide of iron; but it is inefficacious against arsenical preparations, the sulphate of strychnia, and possibly the other alkaloidal salts.—*British and Foreign Med.-Chir. Review*, Jan. 1876, p. 234.

100.—SUCCESSFUL EMPLOYMENT OF PHOSPHIDE OF ZINC
IN A CASE OF MERCURIAL TREMOR, AND IN ONE OF
CHRONIC ARSENICAL INTOXICATION.

By Dr. NOËL GUENEAU DE MUSSY, Physician to the Hôtel
Dieu, Paris.

Phosphorus has taken, during the last few years, a most important place in therapeutics, both as modifying the nervous system and improving its impaired nutrition.

About eight or ten years ago, M. Vigier, a very learned chemist of Paris, pointed out the advantages of phosphide of zinc. This preparation is much more stable, more definite, and more easily handled than phosphoric oil, which is commonly employed. Whilst the phosphoric oil is very easily altered by oxidation, and untrustworthy in its effects, no variation of power, no change of composition takes place in the phosphide of zinc.

Four milligrammes of this substance contain one milligramme of phosphorus, but in reality, according to the researches of M. Vigier, the physiological action is equivalent to only half a milligramme. I always prescribe two pills of four milligrammes each to begin with, and proceed gradually to six pills when the physiological effects, which must be carefully observed, do not seem to be sufficiently marked. If any signs of intolerance appear, I suspend the course for some days, to begin again afterwards in the same way. Generally, after twelve or fifteen days I allow the patient to take rest, and discontinue the medicine during four or five days.

About ten years ago I had occasion to relate in the *Gazette Médicale*, and later on in my clinical lectures, the successful employment which I made of this substance in cases of tremor. A very striking case of this description has recently presented itself to me.

For more than six weeks the patient had been affected with a violent quivering of the limbs, which made him quite an invalid. He could not help himself in any way; and even when his arms were resting, lying on the bed, the trembling did not subside. He also complained of pains in his back, and his face was red and flushed. The gums were red, swollen, and bordered with a purple rim. Fearing, from the spinal pains, that the specific poisoning of the nervous system was attended with a congestive process, I did not venture to give phosphide of zinc immediately, but prescribed a purgative, and applied two small cauteries to the base of the neck. After some days, the pain being relieved and the symptoms of irritation somewhat abated, I prescribed two pills of the phosphide of four milligrammes each. After two days of this treatment, such was the improvement in the patient's health that he could walk alone and eat without being helped; and a few days later he found himself so well, though he was not completely rid of a slight trembling, that he insisted on leaving the hospital.

Another case lately occurred in my wards which gave me an opportunity of testing the modifying action produced on the nervous system by phosphide of zinc. A man of fifty years of age came into my wards complaining of pain and weakness in the back and limbs. He had not been able to walk or stand for one month. The left leg was much weaker than the right. This weakness extended to both arms, but was much more marked in the left one, which could not retain hold of anything, and presented signs of paralysis of the extensor muscles, and especially of the last three fingers. Sight and hearing were also much impaired on the left side. Tactile sensibility of the skin, investigated by means of the *æsthesiometer*, was rather dull, though very slightly so. No alteration was noticed in the perception of heat, or weight, or pain. The vaso-motor action seemed impaired in the integuments of the hands, which were stained of a reddish-blue colour. Pressure on the larger nervous branches, and especially along the vertebral grooves, excited such pain that the patient would start and scream. He had been employed in a manufacture of aniline-red, and had thus been exposed to arsenical vapours. This circumstance, and the recollection of a pamphlet lately published by Dr. Scolosuboff, of Moscow, led me to suspect that the nervous disorders observed in this case might depend on poisoning of the system with arsenic; and, indeed, the analysis of the patient's urine showed the existence of a large quantity of this substance, and thus confirmed my suspicions. I first ordered some baths and the internal use of a mixture of sulphur and honey, in order to eliminate the remains of the poison deposited on the integuments; I then prescribed the phosphorated pills, and

after three or four days I noticed a great improvement in the condition of the patient. He could walk and make use of his hands, the pains were abated, and the general appearance was much more satisfactory. After a fortnight he could distinguish with his left eye small printed characters, whilst he was unable before to make out even the largest type; he was also able to discern colours which hitherto had appeared quite confused to him. The patient is still under my care, and feels every day better, though the strength of the left limbs and of the extensor muscles is not yet quite restored.

In the above case some points deserve to be especially noticed. Paralysis, though general, was more marked on one side, constituting real hemiplegia, and extending to the organs of sense. The very interesting researches made by Dr. Sclo-suboff, with the result of showing that the poison mostly accumulates in the nervous centres, can explain the fact, whilst the rapidity of the patient's recovery indicates that, notwithstanding the gravity of the symptoms, no deep lesion of the organic structures had taken place.—*Lancet*, Feb. 5, 1876, p. 208.

101.—ON THE ADMINISTRATION OF ALCOHOL IN THE TREATMENT OF DISEASE.

By Dr. BENJAMIN W. RICHARDSON, F.R.S., &c.

In the earliest part of my professional career—twenty-five to thirty years ago,—the lessons taught in the English and Scottish schools were very simple and, admitting the premises on which they were based, sound. They were the continuation of a wave of the Brunonian theory, though the name of the theory and much of its curious history had become entirely forgotten. The value of alcohol was thought to lie in its power of sustaining the animal body during “asthenic” states, and of saving the body from exhaustion of its “excitability.” Alcohol, therefore, was administered, with moderate freedom, in cases of general dyspeptic debility; of hemorrhages; of fever, when the pulse was failing in power; of syncope; of shock; of exhaustion from a discharge from the body, as from an abscess, or from free secretion of milk in the woman; of depression from severe inflammatory states, as in carbuncle or erysipelas; of melancholic, depressed, and nervous states of mind; of phthisis pulmonalis; of hysteria; of delirium tremens; of paralysis. It was administered in all cases in which it was considered that the patient would be likely to sink, or in which the patient was thought to be actually sinking into death. The universality of the remedy, as an aid to

substances more purely medicinal, was, in fact, admitted by nearly every practitioner.

A little later, the employment of alcohol in medicine became, I will not say more systematic, but more extended. The teachings of Dr. Todd led many practitioners to "rely," as they expressed it, on alcohol, to the exclusion, in some instances, of all other active treatment. For my part, I was never drawn into the practice of this extreme school; but for more than twenty years I held by those lessons which I originally learned from my first masters.

Within the past six or seven years a change has come over the medical world in respect to the value of alcohol as a remedy. This change is due to the new light that has been thrown upon the subject of the physiological action of alcohol. I do not now enter on the physiological question. It is sufficient for me to say that, without any kind of prejudice against alcohol as a remedy, with indeed some prejudice in its favour, I have felt it a duty to study its medicinal action in a more critical spirit than I did originally, and that the result is a correction of many errors of grave import. Thus I have learned a series of new truths and practices in the treatment of disease which I would submit as being worthy the consideration of those who have not yet arrived at the same conclusions.

To some of the changes of practice I have been led solely by physiological guidance; to some by the process of following and testing the practice of other physicians who have ventured to move in steady advance in the path of clinical research.

Of the new facts which I have, so far, learned from new observation, the following are the most important.

I. There are cases, commonly called cases of debility, in which there is no objective sign of organic disease. The leading symptoms are those of persistent dyspepsia; flatulency; irregular action of the bowels; hemorrhoids; much exhaustion under moderate physical exertion, and great mental depression under slight mental disturbances; extreme nervous excitability, amounting to hysterical excitement; a condition of urine variable in character, the fluid sometimes of straw colour and abundant, at other times scanty and loaded with lithates; the sleep disturbed, with frequent movements and muscular starts of the lower limbs at the moment of going to sleep; a deficient appetite, and a white loaded tongue. In these examples—as common, by the way, to-day as in former times—the old practice used to consist in trying "to regulate" the wine or other alcoholic beverage. My experience now is that these symptoms are in nearly every instance caused by alcohol, and that the only certain successful treatment is total abstinence. To the practical conclusion here stated I have been led by the study of

the action of alcohol upon the healthy body. The phenomena described are the symptoms of alcohol when it is taken in what is commonly presumed to be a moderate, and, as it has seemed to many, a necessary quantity. These phenomena and their cause have been very carefully and ably described by Dr. Marcet.

II. That alcoholic stimulation was the first point of practice in the treatment of acute hemorrhage was a lesson of all others most impressed on my student life. For many years I held by it, as a matter of faith, so strongly, that if in a fatal case of hemorrhage I had been obliged to withhold the stimulant I should have looked back upon the proceeding with sincere regret. At the same time some striking facts were occasionally presented to me, which startled me at the time, and which would have taught to a mind less forcibly impressed with a preconceived idea a new experience. I was called at night to see a weak and emaciated woman who had been attended by a midwife, and who had been flooding for several hours, owing to retained adherent placenta. The woman was semi-conscious, occasionally feebly convulsed, and cold, from the loss of blood. If wine, sherry or port, had been at hand, I should have given her half a bottle at least, or any other form of alcohol in like proportion; but there was not a drop of alcoholic drink in the house, and the house was some miles from a village, so I was obliged to get on without the stimulant. I extracted the placenta, kept up firm pressure on the uterus with the hand, and administered freely the only sustenance that could be had—viz., milk diluted with water and sweetened with sugar. The hemorrhage stayed, the patient fell into a sleep, and I left her in the early morning comparatively safe. At a visit later in the day she was in a condition as favourable as I had ever seen for recovery under like circumstances, and she actually recovered as quickly as could be imagined possible, without taking a single dose of alcohol. This recovery was backed by many others; by the recoveries from exhaustion after venesection without administration of alcohol; by the recoveries of the inferior animals from hemorrhages carried up even to apparent death. They did not teach me anything more than a list of curious exceptions from an imaginary rule.

Meanwhile it was impossible to be blind to another set of facts—namely, that the alcoholic remedy for hemorrhage, potent as it might be, was not without its disadvantages. It caused generally a reaction more or less troublesome; and sometimes, during the reaction, hemorrhage recurred. It caused often an extreme, restless nervousness of the patient. It deranged the secretions. It lessened the appetite for sustaining foods, and it led thereupon to an exhaustive, feverish

condition,—famine fever,—which was not satisfactory. Lastly, it was by no means so invariably successful a remedy as might be desired and expected. These drawbacks, however, pertain to many remedies; indeed, not one remedy is certain, not one perfect. Alcohol therefore, like the rest, must be accepted with all its imperfections on its head.

III. There is a class of cases of the hemorrhagic kind in which the administration of alcohol is, according to my experience, indifferent practice. I refer to cases of recurrent hemorrhage, cases typically seen in simple recurrent menorrhagia in women. The patient under these circumstances, greatly reduced in power, is easily misled by her own sensations to think well of alcohol. She is mentally and physically feeble. She takes, in periods of lowness, a glass or two of wine, or a large draught of stout, and for a time she feels so much relieved by the assumed remedy that she resorts to it again and again, as if it were her only support. There comes a stage at last when this belief is so firmly accepted that nothing, however reasonably or cogently said, can remove it.

The results of the treatment are, at the best, dangerous; at the worst, disastrous. There is engendered a dislike for natural good food, a depraved appetite, and a persistent dyspepsia. The bowels are rendered irregular—at one time sluggish, then relaxed; and the stomach and intestines are frequently distended with flatus. The action of the heart is feeble and irritable. The mind is depressed, and the emotions are either excited into hysterical elation or lowered into despondency. With these symptoms there is the continuance of the hemorrhage, for a time passively, at periods actively and copious. We administer steel, or steel and quinine; we administer mineral acids; we administer gallic acid, or other styptic remedies. We employ astringent injections or other local methods for arresting the loss of blood. All our remedies, useful though they may be, are imperfect, and are slow in their operation, even when they are curative in their ultimate effect.

My experience of this class of cases now is that the first point of practice in their treatment consists in withdrawing the alcohol. As soon as the unrest which alcohol induces is set up, the vascular depression at one moment, the quick excitement at another, is allayed, and the cure commences. Peace is secured. The current of the blood flows on in steady stream; the blood regains its plasticity; the weakened vessels have rest; the nutritive changes are more naturally carried out; the nervous system is toned to even tension; and the signs of amendment begin to appear. Then our remedies seem to be endowed with virtues, and recovery is the natural consequence. I exclude of course from these examples of chronic hemor-

rhage all in which the blood is derived from a malignant source. In these the rule of abstinence from stimulant is good also, but the benefit of it is not to be accepted, as in the other instances, for a method of cure.

Patients suffering from chronic loss of blood, and who are in the habit of taking alcohol to meet the sensation of exhaustion to which they are constantly subjected, are often very difficult of treatment. They hold by the assumed remedy hard and fast, so that it is necessary to be most firm in relieving them from its bad influence. As a general fact, I find that no half measure, no temporising measure, is of avail. If one glass of wine be permitted occasionally, two will be taken, and that means a continuance of the vicious system.

The argument used by the patient against the withdrawal in these cases is special. It is that some kind of undefined but terrible danger must or will occur, if all the so-called alcoholic support is withdrawn. The only danger I know of, and that is real, is to the practitioner, who runs a risk of losing the confidence of his patient if he too determinedly maintains his position. The people generally, even the best educated of them, do not understand this question as we medical men do, and so they persist in defending an engrafted belief which an acquired appetite often sharpens to a very fine edge indeed, with an obstinacy, and, I might say, a skilfulness of argument which is truly perplexing.

The best means of securing confidence is to learn it first; to make sure by observation that the treatment suggested is safe, and having gained this knowledge with certainty, to impart it without a shade of doubt.

It is, of course, a matter of great practical moment to be sure that it is safe to withdraw from a patient that which for a period, and for long periods, seems to confer signal benefit; and for my part, if I had hesitation on the subject, I would express the same. But, so far, I have not the slightest evidence before me of any harm whatever accruing from the entire and sudden withdrawal of alcohol in the class of cases under consideration. On the contrary, when the appetite for the stimulant is appeased, and the mental worry and fear which attend the withdrawal are calmed, I have never seen anything but good as the result of the practice. My experience further is that the lower a patient feels, the greater is the reason, as a general rule, for enforcing total abstinence. The chances are large, that the lowness which the alcohol relieves will be intensified when the effect of the brief stimulation it has produced has passed off.

I have brought forward as types of the chronic hemorrhagic cases those of menorrhagia first, because they are most common.

I include, however, under the same class, others where blood is recurrently lost. Cases of loss of blood from piles are treated with much greater success, and with much more certainty of cure, when total abstinence from all alcoholic drink is enforced. The effect of alcohol is so decidedly to induce congestion of the liver, and venous congestion of the hemorrhoidal veins is so ready a consequence, that many persons I have known who have been subject to bleeding from hemorrhoids have suffered proportionately to the degree of their indulgence. All have been benefited by abstinence.

In the treatment of hemoptysis it has been my practice at every stage of my medical career to enforce abstinence from alcohol as a part of the treatment. This practice I gleaned from an early lesson, and I have never seen any result from it except the best. It is indeed singular to observe that in the treatment of this form of hemorrhage the administration of alcohol is, with an all but common consent, forbidden by practitioners of medicine. In the many cases of hemoptysis I have seen and in the varieties of practice I have witnessed, I cannot recall an instance in which alcohol has been prescribed or recommended in any form as a remedy. I think I may say I do not remember a case in which it has been systematically permitted. That this practice of withdrawing stimulants in hemoptysis prevails so generally is not wonderful. It is begotten of the fear of exciting a recurrence of the hemorrhage by stimulating the heart. It is sustained by the manifest wisdom of the practice, by the observation, so widely confirmed, that in the worst stages of pulmonary disease the hemorrhage ceases under the abstinence, and is in the rarest of instances the cause of death. Nevertheless, it is wonderful that this disease hemoptysis should have been made so remarkable an exception in respect to treatment, and that the success of treating it without stimulation, even when the powers of life are at their lowest ebb, should not have suggested the applicability and soundness of the same treatment in all other hemorrhages, acute and chronic.--*Lancet*, Jan. 1 and 22, 1876, pp. 6, 122.

102.—THE PHYSIOLOGICAL ACTION OF ALCOHOL.

By Dr. T. LAUDER BRUNTON, F.R.S., Assistant-Physician to St. Bartholomew's Hospital, London.

It is only with the action of alcohol more or less diluted on the blood and vessels with which we have to do. It acts on the white blood corpuscles by first increasing and then diminishing their amoeboid movements. What the effect of such increased movements of the corpuscles will be upon the body as a whole it is difficult to say, but the result of its action on the

red corpuscles is more easy to trace. George Harley and Schmiedeberg have found that it lessens their power of giving off oxygen, and must consequently more or less diminish the oxidation of the tissues. Now, both the functional activity of organs and the production of heat in the body depend on the processes of oxidation within them, and it is obvious that any interference with these processes is not likely to be beneficial so long as they are going on in a healthy way, and not too rapidly. As we shall afterwards see, however, this effect is to some extent counteracted, or even more than counteracted, by the action of alcohol in accelerating the circulation, and if the quantity taken be small, and not frequently repeated, little or no harm will ensue. If it be frequently taken, however, by persons in average health, and with fair digestion, its effects will become manifest in the imperfect combustion of fat, and its consequent accumulation in the tissues. This seems to occur especially in the skin, which acquires a velvety feeling. From this quality of the skin I have seen Professor Neumann, of Vienna, diagnose the potatory habits of a man whom one would otherwise never have suspected. If much saccharine or other fat-forming matters be taken at the same time with frequent doses of alcohol the subcutaneous tissue also becomes loaded with fat, as we so frequently see in brewers' draymen, and if the consumption of alcohol be excessive it causes fatty degeneration of various organs. This power of alcohol to lessen oxidation, useless or even injurious in health, increases the value which its other properties give it in the treatment of febrile diseases, where oxidation is going on too quickly, and rapidly destroying the tissues. The very increase of temperature which this oxidation causes helps of itself to accelerate this disintegration of the tissues, for a high temperature causes them to split up, even although they do not undergo oxidation. Thus the albuminous tissues probably become decomposed and yield urea, other nitrogenous substances, and fat. The fat does not undergo complete combustion but accumulates in the tissues from which it has been formed, and thus the heart of patients who have died of pyrexial diseases, instead of being purely muscular, is generally to a great extent fatty.

In such a condition of pyrexia alcohol will diminish the excessive waste in two ways. Firstly, it will impede oxidation, and secondly, by thus lessening the temperature, it will diminish tissue-disintegration.

But while alcohol thus modifies the blood, does it undergo no change itself? Does it simply course through the vessels for some time until it can be eliminated unchanged by the various emunctories, or does it undergo combustion in the blood as the grape sugar from which it is derived would do, and thus deserve like it the title of food?

Great numbers of experiments have been made to decide this question, and diametrically opposite opinions have been founded on them. Liebig classed it as a food along with sugar and starch, and no doubt was thrown on the correctness of this classification, until Lallemand, Perrin, and Duroy published their experiments, from which they concluded that alcohol is entirely eliminated in an unchanged condition, and can therefore, in no sense be termed a food. Their conclusions, however, were much more general than their experiments warranted, and they did not pass unchallenged. What their research actually showed was not that the whole of the alcohol injected passed out of the body, but only that a part of it is excreted. Similar experiments were made by Baudot, who instead of using large doses used small ones, and he found that instead of the whole or a great part of the alcohol being excreted, only a small fraction, so small as to amount practically to nothing, found its way out through the kidneys. The question was then taken up by the late Dr. Anstie, who did much to solve it; and had he only lived to complete the researches on which he was engaged at the time of his death would have settled it completely. His experiments, as well as those of Thudichum, Dupré, and Schulinus confirm Baudot's, and show that only a trifling fraction is eliminated. A year or two ago, Subbotin published some experiments, in which he found a much larger proportion of the alcohol to be excreted than the other observers just mentioned had done, but he, as well as Lallemand, Perrin, and Duroy, used very large doses. Now we all know that grape sugar is a most valuable food—the food we may say *par excellence* of the body, for others are converted into it in the liver—and in moderate quantities it undergoes complete combustion in the body, and is not eliminated in the urine. But a man may be rendered temporarily diabetic by giving him a large quantity of syrup at once, for the organism not being able to consume more than a limited amount at a time, the excess is thrown out by the kidneys. It is therefore not to be wondered at that alcohol should be excreted after large doses have been taken; in fact, the wonder would be if it were not.

The importance of the question whether alcohol undergoes oxidation in the body or not consists in this: if it is oxidized it will supply energy for muscular exertion, or for keeping up the animal heat, or for both, and will therefore be entitled to rank as a food, while if it is excreted unchanged it will have no claim to the name and must be classed with such substances as the organic alkaloids, which after acting on the nervous and muscular systems, while they are circulating in the blood, pass out after a while by the emunctories. It is the merit of Baudot, Anstie, and others who have worked at this subject, to have

shown that alcohol is oxidized, and is thus to be reckoned as a food and not merely as a drug. But still more satisfactory evidence of its claim to the title of food is afforded by the fact that it will keep up the weight of the body and prolong life when the supply of other food is insufficient or is entirely wanting.

Dr. Hammond found that when he took an insufficient diet and was daily losing weight, the addition of alcohol not only prevented this loss of weight, but converted it into an actual gain. In his work on "Stimulants and Narcotics," Dr. Anstie has collected a number of cases in which persons have lived for a considerable time either upon it alone or along with a quantity of food so small as to have been utterly inadequate without it.

From a survey of all the evidence on this subject, I think we may conclude that in moderate doses alcohol undergoes combustion in the body, and will supply energy, yield warmth, and tend to sustain life in the same way that sugar would do, and is therefore to be reckoned as a food. At the same time it has a power of diminishing oxidation which prevents its employment as a food to any great extent in health, but greatly increases its utility in disease.

In feverish conditions it diminishes tissue waste, and thus keeps up strength in three ways:—1. It undergoes combustion itself as a food instead of the tissues. 2. It lessens oxidation in them. 3. It lowers the temperature which itself increases tissue degeneration. It may perhaps seem rather contradictory to say that it undergoes combustion and yet diminishes combustion, but in this respect we may compare it to the sulphur which some people are accustomed to throw into their grate when the chimney takes fire—the sulphur burns itself, but it puts out the blazing soot.

A moderate quantity of alcohol may enable a man to overcome a sudden difficulty, but can its effects be kept up so as to help him with a prolonged effort? Does the alcohol supply new strength, or does it merely enable a man to use up his reserve of energy? If it really supplies strength we ought to find it doing so each time it is administered; but if it merely helps to use up reserve energy, we will find that each successive time it is given the organism responds less and less readily to the call, just as a man gives more and more grudgingly at each successive demand upon his purse. Now the question was thoroughly tested during the Ashanti campaign, and the following are the results as recorded by Professor Parkes:—

"The first effect of alcohol, when given in a moderate dose (for example, what is equal to one fluid ounce of absolute alcohol), is reviving, but this effect is transient. The reviving

effect goes off after at the utmost two and a half miles of additional march, and sometimes much before this; then the previous languor and sense of exhaustion not only return, but are sometimes more intense, and if alcohol is again resorted to, its effects are now less satisfactory. Its reviving power is usually not so marked, and its peculiar anæsthetic and narcotising influence can often be distinctly traced. The men feel heavy, dull, disinclined to march, and are less willing and cheerful."

From this it is evident that alcohol does not impart strength, but rather enables a man to use up in a short time the energy which he usually would have taken a much longer time to expend. If he only requires to make a single effort and can rest afterwards until he has replaced his exhausted store, the additional temporary strength obtained by using alcohol may enable him to overcome an obstacle which would otherwise have baffled him, but if he has to make prolonged exertions alcohol is injurious.

Now the heart seems to be affected by alcohol in the same way as the body generally. No new strength is imparted to it, but it is enabled to draw on its reserve. Thus Parkes has found that when brandy is given to healthy men the pulse becomes quicker, but after the effects of the brandy have passed off it becomes slower than natural, so that the number of pulsations and amount of work done by the heart in twenty-four hours is much the same whether brandy have been taken or not, unless the doses be large and repeated.

The question therefore at once arises,—Is alcohol only useful in stimulating the heart to do additional work, and thus averting the danger of failing circulation for a short time only, or can it be used in diseases where this danger is to be averted for days, and even weeks, together? Before attempting to answer this question, I would remind you that a heart which is beating more quickly than usual, wears itself sooner out than one which is acting slowly. It has been found that a heart which has been made to pulsate slowly for some time by irritation of the vagus, will continue to beat for a good while after it has been removed from an animal's body; while, on the contrary, it very soon ceases to beat if the vagi, instead of being irritated, have been cut, so as to allow the pulsations to be very rapid for some little time before the animal's death. What is observed in these excised hearts is only an exaggerated representation of what occurs in the body, and although in it the pulsations may continue days instead of minutes, yet the final result will be similar. If alcohol always quickened the pulse in disease as it generally does in health it would probably be injurious in prolonged illness, as it was found to be in pro-

longed exertions by the soldiers in Ashanti. But this is not the case, for in fever the quick pulse frequently becomes slower after the administration of alcohol, and, indeed, an excellent rule of practice is not to give alcohol if it increases the rapidity of the pulse already too quick. Alcohol thus economises the vital power of the heart, and tends to prevent death from exhaustion. It is difficult to say precisely how the slowing of the pulse is effected.

The chief points in this paper are:—

1. Alcohol, in small quantities, increases the secretion of gastric juice and the movements of the stomach, and thus aids digestion. Although unnecessary in health, it is useful in exhaustion and debility.

2. It increases the force and frequency of the pulse, by acting reflexly through the nerves of the stomach.

3. In large doses it impairs digestion by over-irritating the stomach.

4. It may produce death reflexly by shock.

5. After absorption into the blood, it lessens the oxidising power of the red blood corpuscles. This property renders it useful in reducing temperature; when constantly or very frequently present in the blood, it causes accumulation of fat, and fatty degeneration of organs.

6. It undergoes combustion in the body, maintains or increases the body weight, and prolongs life on an insufficient diet. It is therefore entitled to be reckoned as a food.

7. If large doses are taken, part of it is excreted unchanged.

8. It dilates the blood-vessels, increases the force and frequency of the heart by its action on the nervous centres to which it is conveyed by the blood, imparts a feeling of comfort, and facilitates bodily and mental labour. It does not give additional strength, but merely enables a man to draw upon his reserve energy. It may thus give assistance in a single effort, but not in prolonged exertions.

9. The same is the case with the heart; but in disease alcohol frequently slows instead of quickening the pulsations of this organ, and thus economises instead of expending its reserve energy.

10. By dilating the vessels of the skin, alcohol warms the surface at the expense of the internal organs. It is thus injurious when taken during exposure to cold, but beneficial when taken after the exposure is over, as it tends to prevent congestion of internal organs.

11. The symptoms of intoxication are due to paralysis of the nervous system; the cerebrum and cerebellum being first affected, then the cord, and lastly the medulla oblongata. It is through paralysis of the medulla that alcohol usually causes death.

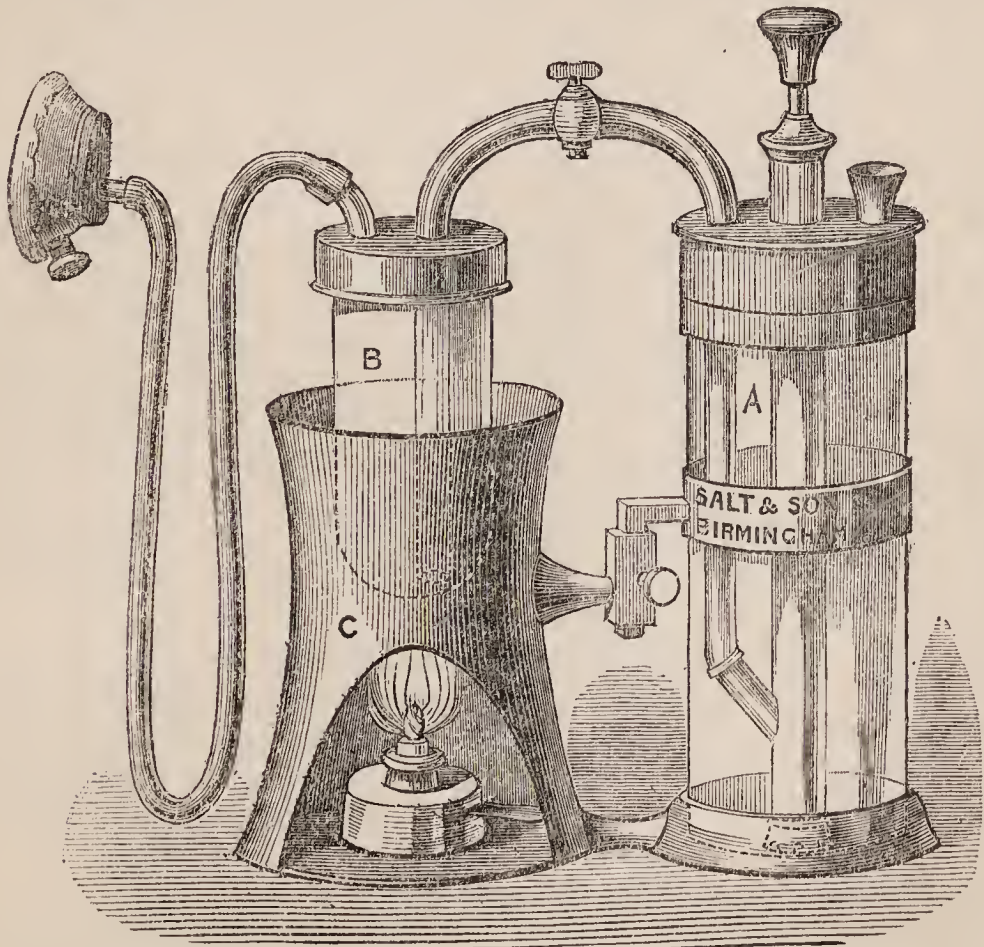
12. The apparent immunity which drunken men enjoy from the usual effects of serious accidents is due to paralysis of the nervous mechanism, through which shock would be produced in a sober condition.—*Practitioner*, Feb. 1876, p. 121.

103.—NEW METHOD OF ADMINISTERING ETHER VAPOUR.

By LAWSON TAIT, Esq., Surgeon to the Birmingham Hospital for Women.

I do not think that any other conclusion can now be drawn from the evidence before us, than that chloroform must be discarded for everything but operations on young children and pregnant women, in both of whom it acts better than any other anæsthetic, and with whom it seems to be wholly devoid of risk. With the single exception of the occurrence of pulmonary inflammation, ether seems free from danger, and therefore its use ought to become general.

Having twice experienced the danger from bronchitis, I tried various plans for removing what I believed to be its cause, and that which I have found to be the best, is to give the vapour



of boiling anhydrous ether, pure and free from any admixture of air.

The woodcut will explain the apparatus which I have devised for the purpose, and which is made by Messrs. Salt & Son, of Birmingham. Anhydrous ether ($\cdot720$) is placed in the reservoir A, which will hold about ten ounces, and which is furnished with a spring pump, which drives over about a dram of ether at each stroke into the glass boiler B. This boiler is suspended in a hot-water tank C, beneath which is a spirit lamp. From the boiler an exit tube, four or five feet long, passes to a Junker's mouth-piece.

When the apparatus is to be used, the tank is filled with water, the spirit-lamp is lighted, and about three drams of ether is pumped into the boiler. Care must be taken that there is no leakage from the boiler, otherwise there will be an explosion. It will soon be found that the boiling ether gives over a large volume of vapour at a constant temperature of a few degrees below the boiling point of the sample, which of course will vary very much, but will generally be found to be pretty nearly the temperature of expired air, 31° — 33° Cels. When given at this temperature, and free from air, the vapour is quite pleasant, and its taste, or rather its comparative freedom from taste, reminded me when I tried it on myself, greatly of the flavour of nitrous oxide. It is, of course, certain that its administration will involve no risk of bronchitis. A few days ago I had to remove the breast of an old lady who had suffered for years from bronchitis and dilated heart. My friend Mr. Priestley Smith gave the ether for me by means of my new inhaler, and we were all charmed by the ease with which the patient passed over into profound coma, and the comparatively small quantity of ether which was needed—about five ounces; and I am pleased to say she has perfectly escaped the complication which I most dreaded—suffocative catarrh. Although the weather had been horribly foggy and cold ever since the operation, she has not had any cough at all, and has been able to lie flat down in bed. From my previous experience, I could not have been induced to allow this patient to incur the risks of the administration of ether in the ordinary way—I should have preferred to use chloroform.

The advance which we have made in our knowledge of anæsthetics since 1849, is in no way commensurate with the enormous amount of labour which has been devoted to the subject. We have, it seems to me, made no advance on ether for surgical work, and Simpson's establishment of chloroform for the alleviation of the pangs of labour stands just where he left it.

I think the method I have detailed will be found to be a substantial advance in the method of ether administration; and if care be taken to keep the boiler vapour-tight, and the mouth-

piece three or four feet away from the spirit flame, I think the apparatus will be found quite safe. Dr. Lauder Brunton made to me the ingenious suggestion of having a steam-jacket round, the boiler, but that I have not found to be practicable without greatly complicating the apparatus. The use of a hot iron bolt to boil the water, instead of the lamp, would probably be safer, but it would not be so handy, and would involve great wear and tear.

I am indebted to Messrs. Salt for the pains they have taken to carry out my plans.—*Practitioner*, March 1876, p. 207.

104.—NOTE ON AN ETHER INHALER.

By Dr. MARTIN OXLEY, Physician to the Liverpool Infirmary for Children.

When in New York this autumn, Dr. Emmett, surgeon to the State of New York Woman's Hospital, showed me an ether inhaler which is the invention of Dr. Allis. It seemed to me at first sight to be the best contrivance for the administration of ether that I had seen, and on trial I have found it quite as perfect as I expected. Its advantages are these:—The ether being very thoroughly mixed with air, the patient does not suffer from the suffocation usually felt at first inhaling; there is a large evaporating surface. A very much smaller quantity of ether is used, and less escapes into the room than with the usual mode of giving this anæsthetic; the ether can be dropped from a bottle on the distal end of the inhaler without removing it from the face; the mask is soft and pliable, fitting accurately to the nose and mouth; and, lastly, it is of very simple construction, and cannot get out of order.

I have given the inhaler a very fair trial, and can confidently recommend it. Mr. Wood, of 81, Church-street, Liverpool, has made a few from the pattern of the one I bought in New York, and has had the framework electro-plated. The inhaler consists of two stout oval frames which are joined together by wire bars three inches long and about one-eighth of an inch apart, making a sort of small birdcage open above and below; through these bars a bandage is threaded, so as to fill up the inside of the cage, the edges of the bandage presenting at its open ends; over this frame is drawn a piece of stout sheet india-rubber, which has been stitched together at the edges, so as to make a covering for the frame, projecting over one end two inches, to form the mask, and at the other one inch. The ether is poured on the bandage, which forms a close, well-made artificial sponge.—*Lancet*, Dec. 18, 1875, p. 874.

105.—ON THE ADVANTAGES OF ETHER OVER CHLOROFORM.

By Dr. JAMES SAWYER, Physician to the Queen's and Children's Hospitals, Birmingham.

[The writer states that until a year ago he always employed chloroform, but during the last twelve months he has always given ether, and that in a considerable number of cases.]

We must answer two questions before we give up chloroform for ether. Can we produce as perfect anæsthesia by means of ether as by chloroform? Is ether safer than chloroform? I believe both these questions may be confidently answered in the affirmative.

No one who has given ether a fair trial, even in one case, can doubt or disparage the perfectness of etherial anæsthesia. I have employed ether very frequently in private and in hospital practice. I have given it for the severest and for the most protracted operations: for ligature of the common carotid artery, ovariectomy, amputation at the hip-joint; and, in eye surgery, for squint and for enucleation of the globe; for excision of the breast, the gouging out of diseased bone, the removal of piles, &c.

But is ether safer than chloroform? This is the most important question. No complete anæsthetic is perfectly safe. We cannot fairly attribute all deaths which have occurred during artificial anæsthesia to the anæsthetic agent in employment at the moment. I wish to make the fullest allowance for all deaths from other causes. I will remind you that the first patient to whom Dr. Simpson intended to give chloroform was a boy who was about to undergo lithotomy. When all was ready, the boy unexpectedly died. No chloroform had been given. Had a few whiffs been taken, death might have been attributed to the anæsthetic. A surgeon, about to perform lithotomy, marked out a line on the patient's skin with his finger, and the man fell dead. Other similar instances might be quoted. But I have seen a hale and hearty labourer, free alike from all emotional excitement and organic disease, laid with a crushed foot upon the operating table; chloroform was given; and before the operation was commenced, without any previous embarrassment of the respiration, the man suddenly died: died because the heart was stopped by chloroform. Of this event I was only a spectator. I believe the man died because the chloroform was given without due dilution with air. In its depressing action upon the heart lies the greatest danger from chloroform. Any one who has given chloroform largely, and with attention to the evidences of its action, knows that it usually diminishes the force of the pulse. In a few

cases, however, in which the pulse is small and weak before the operation from emotional disturbance, the beats become fuller and stronger as insensibility is coming on and the circulation is liberated from emotional control; but, as a general rule, the inhalation of chloroform weakens the pulse. It is often necessary to suspend the administration because the pulse has become feeble and flickering, without the occurrence of any signs of respiratory difficulty. Here, in the fewest words, is a typical case of a cardiac death from chloroform. "A young lad named G. J. Weston, aged 14, died at the Royal Free Hospital whilst under the influence of chloroform. Mr. F. J. Gant, surgeon, said that, when the deceased was admitted, it was thought that the hip had been dislocated. When he examined the lad, it gave him great pain. He ordered the house surgeon to administer chloroform. The chloroform was administered for about two minutes, during which time about sixty drops were inhaled. The deceased seemed to be half-sensible; and, on looking round, they saw the lad fainting and in a dangerous state. Silvester's respiratory method and galvanism were applied, but without effect, for the lad died at once. A *post mortem* examination was made. All the organs were sound. The cause of death was syncope from the effects of chloroform." (British Medical Journal, Dec. 19th, 1874.) I have selected this case, because it is a typical example of death from sudden stoppage of the heart's action during the administration of chloroform. No one can maintain that the degeneration of the heart's walls existed in this young lad. Chloroform, however, like ether, can kill, and has killed, by interfering with the regularity of the respiration, and finally stopping it before the cessation of the cardiac contractions. Death is more avoidable when impending in this manner. The suspension of the inhalation of the anæsthetic, the free admission of fresh air to the lungs, and the performance, if necessary, of artificial respiration, will generally save the patient when dangerous symptoms of respiratory embarrassment show themselves while the heart continues to beat. But we are powerless, or nearly so, when the heart fails first. There is no room to dispute the fact that, in those recorded cases of death from chloroform in which the signs of danger have been sufficiently noted, indications of cardiac failure have greatly prevailed over those of respiratory difficulty.

Just as the great danger of chloroform lies in its power as a cardiac depressant, so does the relative safety of ether consist in its action as a cardiac stimulant. No one can give ether without being struck by its favourable influence over the circulation. In most of the cases in my experience, I have found the pulse improve in force and volume during ether-inhalation.

Mr. Clover, who gave chloroform for many years, and than whom we have no more careful or more experienced administrator of anæsthetics, concerning the comparative safety of ether and chloroform, writes: "Ether is undoubtedly safer; that is to say, life is not so quickly destroyed by it." (*British Medical Journal*, Feb. 14, 1874). In Dr. Neligan's well-known work on Medicines, edited by Mr. Macnamara, I find it stated that "the great recommendations (of ether) are the complete state of anæsthesia it produces, and the safety attending its employment—a safety so remarkable that its exclusive use has become a law in the Massachusetts Hospital."

But we may ask some further questions before we decide to give up chloroform for ether. Are the more remote ill effects arising from ether-inhalation greater than those which spring from the use of chloroform? So far as I have seen, vomiting, the chief troublesome after-effect of anæsthetics, is met with as often after one agent as after the other. Speaking of the remoter consequences of breathing ether, Mr. Clover says: "Sickness is about as frequent as after a moderate use of chloroform." Dr. Neligan mentioned, as one of the disadvantages of ether, "the subsequent persistent taste and odour of ether experienced even for days by those to whom it had been administered." There is, perhaps, a little truth in this: but, with the liberal admission of fresh air, the taste and smell of ether, which are not unpleasant to most people, soon pass away. It has been said that bronchitis may arise as a consequence of ether-inhalation. I have never met with such a result, nor with anything approaching to it. The condition which has probably been mistaken for bronchitis is one which I have observed, and one to which Mr. Clover has drawn attention in the following words. "The flow of saliva is considerable; and the larger air-tubes are often obstructed by fluid, which makes a loud moist rhonchus; but it does not appear to be of much consequence, as it passes off afterwards." Patients who have taken ether usually complain of nothing more than a slight feeling as of recovery from drunkenness. Are there any other inconveniences attending the administration of ether as compared with that of chloroform? It takes a very few minutes longer, as a rule, to produce anæsthesia with ether than with chloroform. But I have induced perfect surgical anæsthesia with ether, so as to permit the painless performance of the operation for squint, in three minutes. Ether is not quite so pleasant to breathe as chloroform, and it is more apt to cause a feeling of suffocation during the first few inhalations. But no inconveniences of such a trivial and temporary kind as those I have mentioned—inconveniences involving time, or taste, or trouble—ought to deter us from using the safest anæsthetic we

possess. If our patients were able to decide the question, safety would be the paramount consideration with them.

I have generally found it best to give a little chloroform at first. One drachm is usually sufficient. I give a drachm as a dose, poured upon a thickly folded towel, held about two inches from the patient's mouth. After this quantity of chloroform has been inhaled, ether can be borne, in most cases, without giving rise to any choking. There is rarely more effect from this quantity of chloroform than a little mental obscurity. I then give the ether, in doses of an ounce at a time, poured upon a sponge, fixed at the top of a cap-shaped inhaler, made from an ordinary bedroom towel. The inhaler I use has been devised and described by my friend Mr. Lloyd Owen. It must be applied closely around the nose and mouth; I like it to be large enough to include the nose and the chin. But little air must be admitted. The dose of ether must be renewed about every two minutes, removing the inhaler completely from the face, and allowing one or two inspirations of air, until anæsthesia be induced. When the insensibility is perfect, it may be maintained by giving smaller doses of the ether, half an ounce, at less frequent intervals. We can only judge whether to stop or continue the administration by the condition of the patient. We must give enough ether to produce anæsthesia, and we must continue to give it so as to maintain anæsthesia, so long as the operation may last. But it must never be forgotten that even ether is not absolutely safe when given to full anæsthesia. The administrator must never cease to watch the patient closely, observing, with undistracted attention, the breathing, the pulse, and the colour of the face; he must have nothing whatever to do with the operation or the operator, nor even to look at it or him; he must be wholly occupied with his own most important and responsible work. Whether chloroform or ether be given, it must be given with carefulness, but yet with confidence; "we must get rid of fear, we cannot act at all till then;" we must avoid alike timidity and temerity.--*British Medical Journal*, Dec. 11, 1875, p. 727.

106.—ON THE ADMINISTRATION OF ANÆSTHETICS.

By J. T. CLOVER, Esq., Chloroformist to the Dental Hospital of London.

1. I am in the habit of using nitrous oxide alone for teeth-extraction and many short operations; and for long ones, I sustain the anæsthesia by first giving ether with the gas, and then ether with very little air. This is very satisfactory on the whole, and has the advantage of shortening the period of recovery, as well as that of going to sleep. I think the uncertain

amount of chloroform in methylene objectionable. I use chloroform with or without ether in operations of the eye, and those of the face or tongue, or in any operation where it is desirable to diminish the hemorrhage as much as possible. 2. I give gas and ether by my apparatus for that purpose. The supply of gas is regulated by my foot moving the screw-tap of a condensed gas-bottle. The ether supply is regulated by the hand which holds the face-piece, by turning the stop-cock leading to the ether-vessel. Ether can be given or withdrawn by this means, without removing the inhaler or letting the patient have fresh air. I use ether *P. B.*, of specific gravity $\cdot 735$. For giving chloroform with or without ether, I use a modification of my bellows and bag apparatus. 3. The favourable opinions as to the greater safety of ether, and the increasing alarm as to chloroform, together with improvements in the way of giving ether, have induced me, within the last four or five years, to give ether very much oftener than chloroform. 4. With the view of securing the safety of anæsthetics, I would advise them to be given to the patient fasting for at least four hours. No stimulant should be given by the mouth previously. Chloroform should not be administered to a sitting patient without great care, nor given in proportions strong enough to excite coughing or swallowing. The pulse, as well as respiration, should be watched; and if there be a pause in the respiration, or if the pulse decidedly lose power, the inhaler should, for at least one inspiration, be taken from the mouth. Raising the chin forcibly away from the sternum, should be adopted when, on account of laryngeal obstruction, the inspiratory movement fails to draw in the proper amount of air. A pause in respiration, after the patient has lost consciousness, should be interrupted directly by compressing the chest and abdomen at the same time every two seconds; but if the patient do not respire independently in less than half a minute, Silvester's method of artificial respiration should be commenced. Ether excites coughing without doing further harm, but should be more diluted after the glottis has become insensible by this or any other anæsthetic.—*British Medical Journal*, Jan. 1, 1876, p. 12.

107.—NELATON'S INVERSION METHOD IN A CHLOROFORM ACCIDENT.

By LAWSON TAIT, Esq., Birmingham.

On July 7th, whilst performing Amussat's operation on a thin, delicate, and much exhausted patient, my attention was suddenly drawn to the fact that the respiration and carotid pulsation had ceased. I tried to feel the beat of the heart,

but, failing to do so I immediately seized the patient round the waist, inverted her and kept her with her thighs bent over my arm and her head hanging downwards. I then directed my colleague in the case to imitate respiration by compressing the chest at intervals of about five seconds. Looking back now at the case it is probable that pulsations began to be felt at the root of the neck in about two minutes after she was inverted, but it seemed to me like a quarter of an hour; and it must certainly have been fully five minutes before she resumed respiration independent of assistance. I kept her in the inverted position for about five minutes longer.

Any one who has seen a death from the uncomplicated action of an anæsthetic must know how useless are the directions for galvanic batteries. Such appliances are sure to be out of order when most needed, and the benefit to be derived from them even when they act is very problematical.

The cases narrated by Dr. Marion Sims were enough to convince me that the inversion method, whatever may be the theory by which its success is to be explained, had rescued patients from impending death; and I am convinced that it saved me from the loss of the patient whose case I have narrated.

Since July I have used nothing but anhydrous sulphuric ether for operations, and though it is far from being so convenient as chloroform, or mythelene ether, I think we are not justified in using any other anæsthetic, save in the cases of young infants and pregnant women, amongst whom no chloroform accident has yet been recorded.—*Practitioner*, Feb. 1876, p. 102.

108.—ON THE ADMINISTRATION OF CHLOROFORM.

By GEORGE H. B. MACLEOD, Esq., F.R.S.E., Regius Professor of Surgery in the University of Glasgow.

For the safe administration of chloroform, the first thing to secure is a pure spirit. I always employ Messrs. Duncan and Flockhart's, of Edinburgh, which has never failed me in efficiency. It is always desirable to prepare your patient, if possible, for the inhalation. A purgative should be administered the day before. If the bowels be loaded, the stage of excitement is always prolonged and excessive. Food should not be given for three hours before the operation; but it is most desirable, especially in weakly persons, to allow them a fair quantity of meat, soup, and bread, at such an interval as I have mentioned beforehand; in private practice, strict and detailed orders are requisite on this point, as unsuitable food is very apt to be given, "to keep up the system." If an undigested meal remain in the stomach, there may be much annoyance and considerable danger from vomiting; while, on the other hand, an

exhausted condition of the system, with diminished courage, and also trouble from flatulence and retching, will arise from too long fasting. A small quantity—a teaspoonful or two—of brandy should be given, undiluted, just before the chloroform is employed. The less bulky the form in which the stimulant is given the better, as it is less apt to be rejected. I have always followed this practice, as I believe that it comforts the patient, aids the effect of the anæsthetic, and helps recovery from it. No special preliminary examination of a patient is made in reference to his getting chloroform; as, if he be fit to be operated upon, he may not only have chloroform, but is thereby rendered more fitted for undergoing the operation. We recognise almost no disease as rendering a patient an unfit subject for chloroform; and to examine him beforehand would only augment his anxiety, and possibly discover conditions which the administrator would better be in ignorance of, as it might render him less decided in his actions, and so the patient might not be as completely under the influence of the anæsthetic as was requisite for his safety. No examination should be capable of augmenting the care and caution always to be employed. Heart-disease, in place of being a counter-indication to the use of chloroform, is often greatly alleviated by its employment; and that fatty change of the organ which is supposed to render it peculiarly liable to “paralysis,” is the very form of disease in which chloroform, if properly administered, interposes the most effectual barrier between it and the fatal shock which an operation is apt to occasion. To obtain this good, however, the patient must be completely anæsthetised. If the action be incomplete, the danger is, if anything, augmented. I have frequently had to perform operations of severity when undoubted heart-disease was present, and no cases seemed to me to do better with it. A good many of the deaths under chloroform have apparently been due to patients suffering from heart-disease not being completely insensible when the operation was performed, and the shock killing them. The chloroform is blamed; whereas what was really wrong was that it was not sufficiently pushed. There are positively no cases which can be submitted to operation in which chloroform is inadmissible. Even in operations about the mouth and throat of the most formidable kind, you will here see it in use. In lithotrity and tracheotomy, also, it is nearly always employed. In some operations, the patient’s help is required for a moment to effect some end; but in general this can be secured before the chloroform is given.

Various things should be placed ready to the hand before you begin to give chloroform to the patient. Here you always see an artery-forceps made fast to the corner of the pillow. I

constantly have one in my waistcoat-pocket, and would never give chloroform without having this instrument at hand. Cold water, and a kettle of very hot water; a bowl with a sponge large enough, when saturated, to fill it full; aromatic spirit of ammonia and a syringe; brandy; and a chair for sitting on, should be at hand. The use of these, I will afterwards explain. Make sure, also, that the window can be opened, and a current of air got.

I prefer giving chloroform early in the day. I have supposed that this renders it less apt to disagree. The patient is to be placed in the recumbent position, with a somewhat low head. Never be induced to administer chloroform in any other posture. Many accidents have arisen from neglect of this. Cover the patient comfortably, but not heavily, and never put any instruments or apparatus on the chest or abdomen. Let there be no crowding of bystanders round his head, so as to obstruct the air. The dress should be loose, especially about the throat; and females should always put off their corsets, and loosen all constricting bands. Any false teeth which may get loose should be removed. A source of great annoyance, and, I am convinced, of danger, is extreme nervousness on the part of the patient. Many, from ignorance, dread the period of insensibility very greatly, and will excite themselves to a great pitch at the anticipation. This is very difficult to allay. They frequently tell you that they know perfectly they will "never come out of it;" and this fear is very often caused by the reading of cases of recent accident from chloroform in the newspapers. Persons who have to undergo operation not uncommonly seek out and store up such knowledge; and I am sure that, if editors of newspapers knew the harm which these otherwise useless records do in increasing the anxiety of both patients and surgeons, they would be more chary of communicating such information to the public. Hysterical anæmic women are most apt thus to cause embarrassment. We all know that excessive fear may of itself kill by syncope; and that it must act most prejudicially on the action of the heart under the circumstances we are now considering, must be conceded. Doubtless, some of the deaths attributed to anæsthetics have been solely due to this mental shock. It is remarkable how completely this alarm disappears after even one experience of the action of chloroform and how quickly the most nervous resign themselves afterwards to its action. A nervous person, though expressing great courage, is generally "a bad breather." He either respire in a shallow ineffective way, or inhales too deeply. Either mode of breathing is objectionable. We wish the patients to respire naturally, as if nothing was being done. We desire, in short, that they think as little as possible about it. Those who breathe faintly

are long in being affected, and take so much of the vapour that they are very apt to suffer from after-sickness; while those who breathe too deeply, will take it more quickly into their system than we may wish. It is well in such cases to withdraw the towel, and quietly show them how to breathe; nay, in persons who are very nervous, it may be desirable to give them this instruction, even some time before they are to be operated on. This may be done in a cheerful encouraging way; they may be even accustomed to the smell of the chloroform if great dread of it be evinced, or a little eau-de-Cologne may be mixed with it to diminish its pungency. It is occasionally necessary, all for the same object, not to inform the patient exactly when the operation is to be performed; and you observe that, in this institution, we generally administer the chloroform to females and nervous subjects in the side room outside of the theatre, so as to avoid the trial of bringing them in before so many. Here, we always use a towel in giving chloroform. No other form of inhaler has ever been employed unless it be Skinner's simple appliance, which, many years ago, I tried without deriving such benefits from its use as to make me abandon the towel, which is so simple, so ready, and so efficient a vehicle. This is Skinner's (shown), and for children it is admirable. This drop-bottle, which allows the easy addition of small quantities, is, however, an essential part of it, and without it the real advantage of the apparatus is not secured. There is, to say the least of it, a mystery about all machines, and they are calculated to produce an apprehension which is undesirable. I have elsewhere seen various forms of inhalers used, and I have imbibed a strong objection to them. The advantage they are supposed to possess is, that they accurately measure the strength of the vapour inhaled, and prevent it from being too concentrated. In order that they should accomplish this, a certain mechanism is necessary which is liable to derangement, and which, I think, does not secure any better supply of atmospheric air than the way in which you see the towel used here. In private practice, an inhaler is troublesome to carry about, and it is expensive; but, above all, many more deaths have occurred where some form of inhaler is used than in Scotland, where a towel is the only agent employed. Do not take a towel with a fringe, as, if it fall over the patient's face, it annoys him and retards his progress. In texture, the cloth should not be too thin, and, when folded, the side without free edges—that is, the one formed by the combined doubling of the whole—should be that on which the chloroform is poured. The administrator's hand is apt to lose hold of one or more of the free edges, if they be placed in front, and these fall down close to the patient's face, and may prove

troublesome. These are trifles which, however, in their sum, count towards the result. We put no oiled silk between the folds of the cloth. The chloroform is to be liberally poured near the edge of the towel, and the cloth so held (all these points were here demonstrated on a patient) that a hand is placed on each side of the head to steady it and keep the neck straight. The towel is then pushed from each side towards the centre, so as to form a gutter in front of the mouth and nose. To do all this satisfactorily, the patient should lie on a narrow bed, where he can be approached on all sides, and the administrator stands directly above his head, looking towards the feet. At least two other assistants are required; one to stand on each side. They attend to the pulse, and are ready to command the patient's movements. There is less anxiety in the use of chloroform in hospital practice than in private life, as there are more assistants and better accommodation in every respect to be had in public institutions. The patient should be made to close his eyes and breathe slowly. Do not at first place the cloth close to the face, but hold it at such a distance as will allow the free inhalation of atmospheric air, so as to give the patient confidence; and then, after a little, it can be approximated. It may be necessary to remove it altogether time after time, so as to allow you to encourage the patient and to prevent panic. Nothing is worse than to force the process. Violent struggling and fainting may be expected if any such dangerous method be pursued. If a child cry, this is a good opportunity to get the vapour inhaled. If the patient be a male, it is useful to expose the pit of the stomach, as the movements of respiration, and frequently the action of the heart, are easily seen in this way; but too great reliance must not be placed in the movements of the abdomen and chest, irrespective of other evidence of the breathing going on. The administrator must, however, watch these movements intently, and it is not difficult for him at the same time to feel the temporal artery with the fingers which are below the towel. He must constantly, as the patient is become affected by the anæsthetic, look at the face and lips; it is there that he will at once detect any abnormal condition. Whenever the respiratory function is becoming embarrassed, the dark colour of the lips will at once give evidence of it; while, if the heart's action threaten to fail, the blanching of the lips will at once attract his attention. In old and weak people, and those who are anæmic from any cause, special attention is called for. It is always to be borne in mind that both the breathing and the pulse demand attention; for, although I very strongly incline to the belief that obstruction to the respiration always precedes the syncope of which we have heard so much, still the observa-

tion of competent and careful administrators has not so entirely confirmed that view as to entitle us to concentrate all our attention on the breathing alone. The sound of the respiration should, if possible, be constantly noticed, and all snoring or difficulty at once looked to. It is true that ordinary snoring may occur ; but stertor or the snorting of obstructed breathing is a warning never to be for a moment neglected. It seems likely, from the observations of Mr. Lister, that stertor may have both a palatine and a laryngeal origin ; but whenever it occurs, it is a warning-note of approaching danger.

I should have said before that we never measure the amount of chloroform poured on the cloth. We do not believe that it matters much what quantity is used, so long as the effects are watched. More deaths, I fear, are due to too little than to too much chloroform being given. By this I mean that in very many cases the fatal result has been due to imperfect narcotism, and the shock of the operation having full or even augmented force on the patient. Never trust for safety to the smallness of the quantity employed. Drops may kill when drachms might save. No more erroneous proof of the efficiency of the method of administration used can be than an appeal to the small quantity which has been expended ; and yet, in fatal cases, how often is that advanced as evidence of the great care which was exercised !

When renewing the chloroform, do not remove the towel, but pour it on the outside, and turn the cloth. In this way, much time is saved, and less chloroform is, on the whole, administered, as the patient is not allowed half to recover each time that more chloroform is required. Never, under any circumstances, lay the towel down on the patient's face without your hand being below it.

To keep up the effects, the administrator must carefully watch for any evidence of returning motion ; and, by the reapplication of the cloth, a mere sniff of the vapour may be enough to retain command of the condition in which we wish him to remain. The less chloroform that is used to accomplish this the better.

Perfect quietness should be maintained while the administration is proceeding. The patient's attention is excited by any talking or other noise, and he often strains to hear what is said (supposing, if he be at all nervous, that it is sure to be about himself); and in this way the influence of the vapour is delayed. —*British Medical Journal*, Jan. 1, 1876, p. 5.

109.—ON RAILWAY INJURIES.

By JOHN ERIC ERICHSEN, Esq., Senior Surgeon to University College Hospital.

[From a very able review of Mr. Erichsen's work on Railway Injuries, Concussion of the Spine, &c., in the British and Foreign Medico-Chirurgical Review, we give the following important remarks.]

In the scientific, cautious, and philosophical work which heads this article Mr. Erichsen tries to classify and arrange these anomalous cases, and endeavours with very considerable success to group the various phenomena in something like a natural order or sequence.

The real practical difficulty in the whole question seems to be the explanation of the primary symptoms of concussion or wrench of the spine in those cases in which we have no evidence either of fracture or dislocation of the vertebræ, of separation or laceration of ligaments, of rupture of muscle, or hemorrhage, into its substance, or even of hemorrhage into the membranes or substance of the spinal cord. By exclusion we can get rid of each and all of these symptoms, and yet there are patients to whom something has happened, very difficult to explain, and yet evident in its results.

What this something is we do not know. Mr. Erichsen tries to give us an idea of it by an analogy. When a magnet is struck a heavy blow with a hammer, the magnetic force is found shaken or concussed out of the horseshoe; we do not know how this is done, but we know that it is so, and that the iron has lost its magnetic power. So if the spine is badly grazed, shaken, or concussed by a blow or shock of any kind communicated to the body, we find that the nervous force is to a certain extent shaken out of the man, and that he has in some way lost nerve power.

What immediate change, if any, has taken place in the nervous structure to occasion this effect, we no more know than that change happens to a magnet when struck. But we know that a change has taken place in the action of the nervous system, just as we know that a change has taken place in the action of the iron by the loss of its magnetic force.

With reference to the effect supposed to be produced on the nervous system of the cord by the vaso-motor system producing contraction of the blood-vessels, the following interesting and suggestive passages may be quoted from Dr. M'Donnell. "When we see a large snake struck across the tail with a rod, and instantly, in the twinkling of an eye, seized with a paralysis as complete as death, which yet after a time passes off again, can we attribute such a condition to the constriction of the

blood-vessels of the cerebro-spinal axis? I think not; because we know that among these animals the cerebro-spinal system does not (even after the evacuation of the blood of the body) speedily cease to exercise certain functions and perform movements; and moreover, I have proved experimentally that even after decapitation a blow across the tail stops for a while all the movements which, under ordinary circumstances, persist for a considerable time. The effect, therefore, cannot be attributed to disturbance through the blood-vessels; it appears rather to be due to an altered molecular condition of the nervous centre resulting from the blow, and more or less persistent. That a shock conveyed to the central nervous system through its peripheral nerves should, with the instantaneousness of a lightning flash, lock in insensibility and motionlessness the entire frame of the creature, cannot, to my satisfaction, be accounted for through the action of the vaso-motor trunk upon the blood-vessels."

If this is not a theory it is an analogy, and a good one, and may be a comfort to us, but still it does not explain why it is that this extraordinary change does not occur in cases where the stroke or jar has been of severity sufficient to crush the spine or fracture a thigh or the base of the skull.

A stable-boy in love let himself down from a parapet of a very high bridge, and fell on his feet, breaking both his thighs; he never had a spinal symptom. A drunken workman leapt clean out of a fifth-storey window, and fell on his heels with such force that the bones of the heel of both feet were pulverised, yet he had not a spinal symptom.

Mr. Erichsen comforts himself again with an analogy, which for the same circumstances was a great favourite of the late Mr. Syme. If a watch falls to the ground, and the glass is broken, the mechanism rarely suffers injury, and the watch may not even stop; if the glass escapes, the jar will stop the watch, perhaps even be sufficient to spoil its regularity for ever. In the case of the spine, the violence of the shock may have expended itself in producing the fracture or the dislocation, so that the nervous system escapes the jar.

There is in all probability some obscure change in the mutual relation to each other of the cells or ultimate structures of the nerve-tissue, a change which takes time to develop itself into what we can see by the microscope or trace by symptoms.

What, then, is the series of phenomena destined to end in such serious results? How are we to recognise such a case so as to be able to prognose it? Could we prevent the serious results did we recognise the nature of the case in time?

Unfortunately at the time of the accident the sufferer is usually quite unaware that anything out of the common has

happened to *him*. His attention has been taken up entirely with the circumstances of the collision. He has felt the jolting of the carriage before it left the rails, or has found himself on an embankment, or under a mass of *débris*, almost without knowing what has happened. He is perhaps so delighted at having escaped with his life, and without broken bones, that in all probability he is in a preternaturally excited, even cheerful condition; runs about assisting every one, telegraphs that he has made a wonderful escape, shows no evidence of paralysis, concussion, shock, or strain, and may even take a long journey to reach his home. Then, Mr. Erichsen tells us, "a revulsion of feeling takes place, he bursts into tears, becomes unusually talkative and is excited. He cannot sleep, or if he does he wakes up suddenly with a vague sense of alarm. The next day he complains of feeling shaken or bruised all over, as if he had been beaten, or had violently strained himself by exertion of an unusual kind. This stiff and strained feeling chiefly affects the muscles of the neck and loins; sometimes extending to those of the shoulders and thighs. After a time, which varies much in different cases, from a day or two to a week or more, he finds he is unfit for exertion and unable to attend to business. He now lays up, and perhaps for the first time seeks surgical assistance." Pp. 157, 158.

It is difficult to improve upon, or to correct this excellent description; but in our experience we have occasionally found the stage of exaltation and excitement last for a longer time before the patient begins to discover that he has been injured, and it is quite possible that for the first two or three nights after the accident the sleep may be sound and refreshing. Still the patient is not himself, he is what the Scotch call *fey*, excited, absurdly cheerful; in an altogether exalted frame of mind, and the longer this stage lasts, the more complete and prolonged is the subsequent condition of depression and gloom.

Passing on to the pathological changes in the cord which lead to, or end in, more acute symptoms, Mr. Erichsen is inclined to distinguish two distinct and, indeed, widely opposed conditions, viz., spinal anæmia and spinal inflammation. Though apparently so different in character many of the symptoms of these conditions are alike, the *anæmia*, however, being in all probability a functional, and thus possibly a temporary, condition, the inflammation having organic and permanent changes in the cord substance underlying it.

Inflammatory changes of a chronic, or, at least, subacute character may come on gradually and insidiously, probably within a year from the concussion. In all probability both cord and membranes will be involved in the mischief, though Mr. Erichsen, following Ollivier, tries to differentiate the symp-

toms of spinal meningitis from those of myelitis. As a result of both, the change in the cord is most commonly a softening, perhaps, of the white matter only, but sometimes one of induration and enlargement.

For various reasons, well given by Mr. Erichsen, there is a great lack of records of examination of the cords of those dying after spinal concussion from railway injury. One only is to be found, and this is a valuable one, from the care with which the case was observed by Mr. Gore, of Bath, and the detailed report of the examination of the cord by Dr. Lockhart Clarke.

The essential points in his report are partial thickening of the membranes and occasional adhesions to the white columns; a diminution, to a most striking extent, of its antero-posterior diameter; disease of the posterior white columns, the nerve-fibres of which had been to a certain extent replaced by compound granule-corpuscles and wavy fibrous tissue. The case, in many respects, resembled one of locomotor ataxy.

If the inflammation of the membranes is at all severe or extensive it is sure to affect the spinal nerves, either by causing undue pressure on them at their exit from the sheath, or, less likely, by extending down the neurilemma. These pains are connected with rigidity, contraction, or painful movement of the muscles of the limb.

In endeavouring to distinguish the symptoms of spinal myelitis from those of meningitis, which, as we have said, is only possible in early stages, and is not at all certain in any stage, we may, perhaps, expect cerebral symptoms to occur more rarely and pressure to elicit more suffering in the myelitis than in the meningitis. We may quote here from Erichsen the more characteristic symptoms of myelitis:

“The sensibility is first augmented, but after a time becomes lessened, and gives way to various uneasy sensations in the limbs, such as formications—a feeling as if the limbs were asleep (*engourdissement*). These sensations are first experienced in the fingers and toes, and thence extend upwards along the limbs. These sensations are most complained of in the morning, soon after leaving bed. They intermit at times, fluctuating in intensity, and in the early stages are lessened after exercise, when the patient feels better and stronger for a time, but these attempts are followed by an aggravation of the symptoms. Some degree of paralysis of movement, of loss of motor power, occurs in certain sets of muscles or in one limb. Thus the lower limbs may be singly or successively affected before the upper extremities, or *vice versâ*. Occasionally the loss of power assumes a hemiplegic form. All this will vary according to the seat and extent of the myelitis. There is

usually constipation in consequence of loss of power in the lower bowel. It is very rare that the bladder is early affected, the patient having voluntary control over that organ until the most advanced stages of the disease, towards the close of life, when the softening of the cord is complete. Ollivier remarks that in chronic myelitis patients often complain of a sensation as of a cord tied tightly round the body. The gait of patients affected with chronic myelitis is peculiar. It is unsteady, rolling, like that of a partially-intoxicated man. The foot is raised with difficulty, the toes are sometimes depressed, and at others they are raised, and the heel drags in walking. The body is kept erect and carried somewhat backward."—Pp. 186, 187.

So far, then, we can recognise, with some distinctness, the symptoms of the second stage in those cases of concussion of the spine which result in a chronic inflammation, either of the cord or its membranes or both combined. A careful examination into the history and the objective phenomena, of temperature, pulse, contractility, and firmness of muscle, will generally enable the surgeon to come to a pretty accurate diagnosis, and thus to give a tolerably certain opinion and prognosis, which latter is generally an unfavourable one.

We must now attempt a much more difficult task, to follow our author in his description of the symptoms of spinal anæmia and its results, which are with great difficulty differentiated from those of so-called hysteria.

Occurring, as it does, chiefly in the young, from fifteen to thirty-five, and in a large proportion of cases in females, who had, perhaps, been always somewhat weak and excitable, this malady is apt to be mistaken for or confused with hysteria in one or other of its Protean aspects.

The chief symptoms of this malady, as given in the manuals and homologated by Mr. Erichsen, are—

Pain, induced or increased by pressure on or near the spine, and aggravated by the contact of a hot sponge. This is not permanent, is most marked when patient is expecting to be touched, is perhaps absent entirely when patient's attention is diverted.

Paralysis, in bad cases, of the whole lower limbs, which are flaccid, cold, and insensible. Erichsen says—"The condition is, in fact, one of complete exhaustion of the spinal system below a certain level, that level usually corresponding with a line drawn round the body from the tenth dorsal vertebra."

Special senses, such as *sight* and *hearing*, are often seriously impaired and temporarily weakened, or even destroyed. From the fact that this condition is never in itself a fatal malady, no post-mortem examinations are recorded.

Allied to this condition so closely as with great difficulty to be distinguished from it are found cases of mental rather than physical perturbation, which, for want of a better name, Mr. Erichsen calls hysteria, which he has never seen as results of any of the ordinary accidents of civil life, even in females, but of which he has seen many examples, in both sexes, after the more severe types of railway collisions. Whether such cases are caused by the terror and suddenness of the accident affecting the mind only, or by any special vibratory thrill acting physically on the nervous system itself, it is impossible to determine. Probably the former is the more rational explanation, as the symptoms are mental rather than physical, and they are not apt to be followed by inflammatory or degenerative changes in the substance of the cord.

We will not quote Mr. Erichsen's eloquent account of these symptoms; they are, in a word, an exaggeration of all the subjective symptoms of spinal injury, with a "nervous mimicry" of numerous diseases, aggravated by examinations, especially by the surgeons to the railway company, not in the least improved by treatment, but rapidly tending to get better after a certain number of months, and especially after the worry and anxiety of litigation is fairly over. Can we wonder that, such being the case, invidious and uncharitable remarks are made by lawyers and the suffering railway companies? and yet all through the case the *bona fides*, both of the patient and her medical advisers, may have been unimpeachable.

Mr. Erichsen would base his attempts at the diagnosis of these perplexing cases from those depending on real structural brain-lesions on three chief points:—1. That the mental condition had come on almost immediately after the injury, and has been continuous, not progressive. 2. The pain has a peculiar character of cutaneous hyperæsthesia, which is unattended by alteration in texture, loss of movement, or muscular rigidity. 3. The wonderful good health, notwithstanding the long idleness and mental vacuity.

The prognosis in these cases is generally favourable.

Mr. Erichsen considers the various forms in which injury of the eye or impairment of vision may appear under four well-marked and distinct heads.

1. Concussion of the eyeball and direct shock to the nerve; such injuries may be inflicted during a railway accident and the patient may hardly be able to tell how. Mr. Erichsen believes, and supports his belief by one well-marked case, that a smart, direct blow on the eyeball may at once paralyse the retina without giving rise to any organic mischief, laceration of tissue, or effusion of blood in the eyeball itself. Such cases of simple shock are most probably rare, there being more

usually some effusion or laceration of the iris, choroid, or ciliary processes; in many cases some effusion of blood into the vitreous humour. Again, under this head are included cases of cataract developing after a blow on the forehead or eye-brow, as a result of nerve-injury.

2. *Nerve-injury* may also explain cases of extreme mydriasis, and consequent impairment of power of accommodation, seen after blows or falls implicating the forehead and eyebrows. Some of these cases are remarkable and inexplicable, especially as to why some cases suffer from eye-symptoms and others do not. A slight blow, which has hardly made a mark on the forehead, in one case setting up a slow and gradual process ending in total blindness; another, much more severe one, having no effects at all. A case was recently observed by the writer in which a fall from a height of between 60 and 70 feet inflicted cuts of great severity over branches of fifth, both above and below orbit, and caused so great a shock as to fill the sub-conjunctival tissue with ecchymosed blood, and yet not a single symptom of impairment of vision followed; while many cases are on record in which amaurosis and other eye symptoms have followed comparatively slight injuries. Among other papers on this subject we may refer to a full series of such cases reported by Mr. Benjamin Bell, Surgeon to the Edinburgh Eye Infirmary.

The ophthalmoscope has cleared up the diagnosis and greatly facilitated the prognosis of many of these cases, explaining the symptoms by subretinal effusions, lacerations of edge of the iris, &c.

3. By far the most important, insidious, frequent, and misleading symptoms are those directly caused by the spinal injury itself quite apart from any local injury to the eye or nerves—gradual weakness of vision, with a want of definition, *not improved* by glasses, passing into double vision, with photophobia, and even to the presence of permanent muscæ, flashes of light, sparks, and other subjective phenomena. Mr. John Tweedy has given an explanation of the symptoms under four heads. (a) Asthenopia, with want of accommodation-power for near objects for more than a few minutes at a time, the result either of impaired nerve-supply or of weakness of the ciliary muscle or internal recti. (b) Amblyopia, a paresis of the retina or optic nerve, rendering it incapable of receiving or transmitting impressions to the brain. (c) Anomalies of accommodation or refraction, hypermetropia, myopia, or astigmatism, if of sudden or rapid occurrence, are all grave symptoms. (d) Irritability from hyperæmia of retina, and inflammation of it or optic nerve.—*British and Foreign Medico-Chirurgical Review*, Jan. 1876, p. 4.

110.—ON PROFESSIONAL MUSCULAR ATROPHY.

By Dr. E. ONIMUS, Laureate of the Paris Academy of Sciences.

Activity of muscles determines the development and energy of muscular fibres, and the general law is that the more a muscle works the larger and the more powerful it gets. This law, however, has its limits, and I have just observed a certain number of cases in which the exaggerated work of certain muscular groups, far from producing hypertrophy, induced, on the contrary, a condition of considerable atrophy.

These cases are observed only in individuals who, through the nature of their trade or work, are obliged to contract the same muscles constantly. Through excess of activity, irritation of the muscular fibres supervenes. Thus, in a man employed in a draper's establishment, and whose business was to replace the unfolded goods on their shelves, there supervened, little by little, a most remarkable atrophy of the deltoid muscles of both sides. And, indeed, it was these muscles which were constantly actively employed in performing this special work.

In a workman employed in a tannery, who was every day for eleven hours at work, and always felt aching and fatigued after his day's labour, there likewise supervened marked muscular atrophy, confined to certain muscles. In order to prepare the skins, he was to perform with both arms a forward and backward movement, which necessitated especially the action of the muscles of the shoulder, so that these were the first to be affected, and are at present almost completely atrophied. The wasting away is almost the same in both arms, as both were in action during the man's work, whereas, in respect to the legs, the right one alone was obliged to support the whole weight of the body. Consequently, with the lower limbs, the right leg is the only one that has wasted; it is one-half smaller than the other, and the affected muscles are those the action of which was the most constant, such as the rectus femoris, vastus externus, and vastus internus.

In the beginning, the patients complain especially of prostration, of weakness even in the morning on getting out of bed. They feel—particularly at the outset of the disease—intense, darting, intermittent pains. Before atrophy is well marked there always exists more or less temporary contraction of the muscles.

When wasting has once begun, it follows a most rapid course if the patient continues to fatigue his muscles.

Almost always this affection is mistaken for progressive muscular atrophy, but it differs from it in its course and in a great many of the symptoms. 1st. The muscles which are the first to be affected are generally the largest ones, and particu-

larly those in the neighbourhood of the shoulder-joint. 2nd. The pain and cramps at the outset are also a distinctive sign. 3rd. These cases of wasting amend rather rapidly under the influence of rest and the use of constant and continuous electric currents.

Recently I observed one case which it was most difficult to differentiate from progressive muscular atrophy, as the atrophied muscles were the same as those which are the first affected in this latter affection. They were the muscles of the thenar eminence, and chiefly the adductor pollicis. The patient was an enameller, who had to hold an object all day between his thumb and index-finger. He first got cramps in the thumb, which suggested the idea of scrivener's palsy, then tremor of the thumb, on account of the fibrillar contractions, and, lastly, atrophy. Under the influence of treatment there was a rapid amendment, which showed that the case was really one of professional muscular atrophy, and not commencing progressive muscular atrophy.—*Lancet*, Jan. 22, 1876, p. 127.

111.—ON THE EXTERNAL USES OF HYDRATE OF CHLORAL.

By Dr. WILLIAM CRAIG, F.R.S.E., Lecturer on Materia-Medica, Edinburgh School of Medicine.

[In addition to its hypnotic effects chloral is a powerful antiseptic, and may be used for a variety of purposes, such as the preservation of anatomical preparations, the injection of bodies for the dissecting-room, and for the dressing of wounds. The following are the conclusions arrived at with regard to its value for *dissecting-room purposes*.]

1. Bodies injected with the hydrate of chloral are preserved from decay equally well as when the ordinary preservative fluids are used.

2. If exposed to the air, or carelessly attended to by the students, the tissues become very black, and give off a disagreeable mawkish odour.

3. Delicate nerve-plexuses can be more successfully dissected out when the bodies are injected with chloral solution; and,

4. It is much cheaper than other preservative fluids.

[A solution of 5 grains of chloral to the ounce of water preserves *anatomical preparations* better than spirit.]

As a dressing for wounds and ulcers.—I have tried chloral extensively as an external application to wounds and abraded surfaces. I found as the result of these experiments that a lotion containing from 5 to 15 grains of the hydrate of chloral to the ounce of water formed an excellent dressing to ulcers and wounds, dressed with lint and gutta-percha in the ordinary

manner. I could relate several cases, but I will select only one. A young lad, T. M., lately one of the boys in the Mars training-ship, had one of his legs severely burned, and after being treated by the surgeon in charge of the ship for three months, the boy was recommended to go into Dundee hospital to have the limb amputated below the knee. As T. M. was within a fortnight of receiving his discharge from the ship, he was allowed to visit his friends in Edinburgh, who were unwilling that the leg should be amputated, and put the boy under my care. I saw him in April last, and found a large ulcer on the leg, extending from a little below the knee to the middle of the foot, and several inches in breadth. The edges were very irregular, and a considerable amount of foetid discharge came from the ulcer. The boy and his friends were very anxious that I would try and preserve the limb. I ordered a lotion containing 15 grains of the hydrate of chloral to the ounce of water, some chloral lint, such as is manufactured by J. P. Macfarlan and Co. of this city, as recommended by Dr. P. H. Watson, and some gutta-percha, and gave instructions for having the limb dressed twice daily with these. I may mention that previously it had been dressed with some preparation of carbolic acid. After the chloral dressing the limb healed rapidly. The ulcer got gradually less, the foetid discharge disappeared; and when I saw the patient in July last, only a small ulcer remained, and even that was gradually diminishing in size, notwithstanding the fact that the boy was daily employed in a large drapery shop in town, and was unable to give the leg that rest which was necessary, and which I had so much recommended. The boy is now a sailor, and is at present on a voyage to Athens. I also used chloral solution as an injection into the sacs of large abscesses, and found that it tended much to diminish secretion and make the parts heal. I found it also a useful lotion for the eye in inflammatory conditions of that organ. It is an excellent application to burns, and very specially where there is a foetid discharge. I also found it a good application to remove warts from the hands and fingers. I used for this purpose a lotion containing 15 to 20 grains to the ounce of water, applied by means of lint and gutta-percha. It causes no pain, and the wart speedily becomes smaller, and gradually disappears.

I also used it as a lotion to sore nipples and to inflamed mucous membranes. When chloral is applied to an ulcer, a wound, or to the interior of an abscess sac, it causes at first some smarting, but that only lasts for a few minutes, and is soon succeeded by a most agreeable sensation. Patients so treated have frequently told me that soon after the lotion was applied a very agreeable soothing effect was felt in the wound. I believe that in all such cases chloral acts as a local sedative.

It produces anæsthesia of the nerves of the part. Wherever there is a wound or ulcer there is irritability of the nerves of that part; and chloral, by soothing this irritability of the nerves, favours the healing process.

I have frequently used with good effect an ointment containing 30 to 60 grains of the hydrate of chloral to the ounce in eczema and other allied affections. I believe it to be one of the best applications in such diseases; and a medical practitioner lately told me that he had used it with marked benefit as a local application during an attack of erysipelas of the head. Chloral in various forms has been extensively used in the Royal Infirmary of this city by Dr. P. H. Watson, senior surgeon to that institution, and I have the honour of appending a letter from him giving an account of his experience of hydrate of chloral as a dressing to wounds:—

“I have in my wards made use of the chloral hydrate for fully six months, and find it quite as active as an antiseptic as carbolic acid or boracic acid. It approaches nearer to carbolic acid in its effects than to the boracic acid, especially in that it is volatile, and thus by its vapour penetrates and surrounds parts to which as a dressing it has been applied with an atmosphere of itself.

“It has a marked advantage over carbolic acid, in so far that its odour is pleasant, resembling some of the ethereal compounds employed for flavouring purposes. It also is absorbed, and, in being so, deadens pain after an operation.

“I employ it in *four* forms:—

“1. A lotion of 5 to 40 per cent. in water, for cleansing away discharges around a wound, cleansing sponges used in operations, and analogous purposes.

“2. An ointment composed of concrete paraffin, white wax (Scotch), and almond-oil, to which 1-12th to 1-8th of chloral is added, while the other ingredients are liquefied by heat. The components of the ointment should at once be rubbed together, covered, to prevent the evaporation of the chloral, and cooled to a concrete form as rapidly as may be. It is afterwards rubbed up with a few drops of the solution of chloral to disintegrate it, and prevent its crystalline form being reassumed. This ointment takes great pains to make efficiently. If not properly prepared, it is either inert, containing sometimes, I find, absolutely no chloral; at other times (if made cold) the chloral is so imperfectly mingled that it acts as an irritant, and blisters tender cutaneous surfaces. The ointment is applied spread into the substance of linen cloth, so as to be incorporated with the material. This dressing forms the immediate application to the surface around the wound, and covers in the wound itself. It does not adhere, but peels off like a thin layer of wax.

“3. An external excipient dressing is made by soaking lint in a solution of chloral (3 i. ad 3 j.). It is then wrung out of this and carefully dried. The care is necessary to avoid long exposure or a high temperature, as this volatilizes the chloral.

“4. Lint soaked in a solution of chloral in olive-oil (1-8), employed to fill cavities such as those left in some excisions, and to employ as compresses when it is desired to prevent bleeding from the cut surfaces in operations for the removal of dead bone.

“In some cases, when the chloral appears to act as an irritant, even when carefully prepared, it may be necessary to interpose some impermeable material between the line of operation and the dressing.

“I have never met with any disagreeable results from the absorption of the chloral. On the contrary, I have found the pain of recent wounds only satisfactorily modified and relieved by its employment.”—*Edin. Med. Journal*, Feb. 1876, p. 711.

112.—ON THE DANGERS TO THE EAR OF BREATHING BY THE MOUTH.

By Dr. GUYE.

At the international medical congress in Brussels, in 1875, Dr. Guye read a paper in the otological section (*Archiv fur Ohrenheilkunde*, February 4, 1876), on the dangers to the ear of breathing by the mouth. According to the author, the habit of breathing by the mouth, frequent among deaf people, is a very pernicious one, and he is much surprised at the almost entire absence of papers on the subject. Respiration by the mouth, which is easier than respiration by the nose, cannot with safety be substituted for it, as nasal respiration answers certain requirements which oral breathing cannot supply. The function of the nose in respiration is threefold. 1. The olfactory sense secures it against the entrance of impure air. 2. The moisture of the nasal passages gives a certain degree of aqueous saturation to the inspired air, the contact of which is thus rendered less irritating to the mucous membrane of the throat and larynx. 3. The inequalities of the organ retain solid particles suspended in the air, which is proved by the quantity of dust sometimes found accumulated in the nostrils.

These advantages are absent in respiration by the mouth. The contact of dry air soon produces circulatory troubles in the pharyngeal region, and even an habitual catarrh, susceptible of easy transmission by continuity to the Eustachian tube and cavity of the tympanum. Granular or adenoid pharyngitis often has this origin. In support of his opinions the author adduces the authority of Dr. Paul Niemeyer of Magdeburg, who considered that the attacks of pseudo-croup, to which chil-

dren are often subject during the first hours of the night, had their origin in dryness of the glottis produced by oral respiration. To obviate this inconvenience the principal object was to restore the nose to its proper condition, and for this purpose it is important to oblige the patient to breathe by the nose. As we cannot rely on the will of the individual, especially if a child, Dr. Guye proposes to produce occlusion of the mouth by a little instrument of the shape of the respirator, but differing from it in this essential particular, that it is entirely impermeable to air. This "contra-respirator," as the author calls it, constitutes a simple and easily-applied means which has produced excellent results in Holland. Dr. Guye cited cases of catarrhal deafness which he had completely cured by this means alone, without any other treatment. [The danger to the Eustachian tube of breathing by the mouth was recognised by Mr. Toynbee, who, in his work on the Diseases of the Ear, in speaking of the treatment of obstruction of the faucial orifice of the Eustachian tube by thickened mucous membrane, advised the patient to practice nasal respiration.—*Rep.*]—*London Medical Record*, March 15, 1876, p. 130.

113.—DESCRIPTION OF A FORM OF STRETCHER.

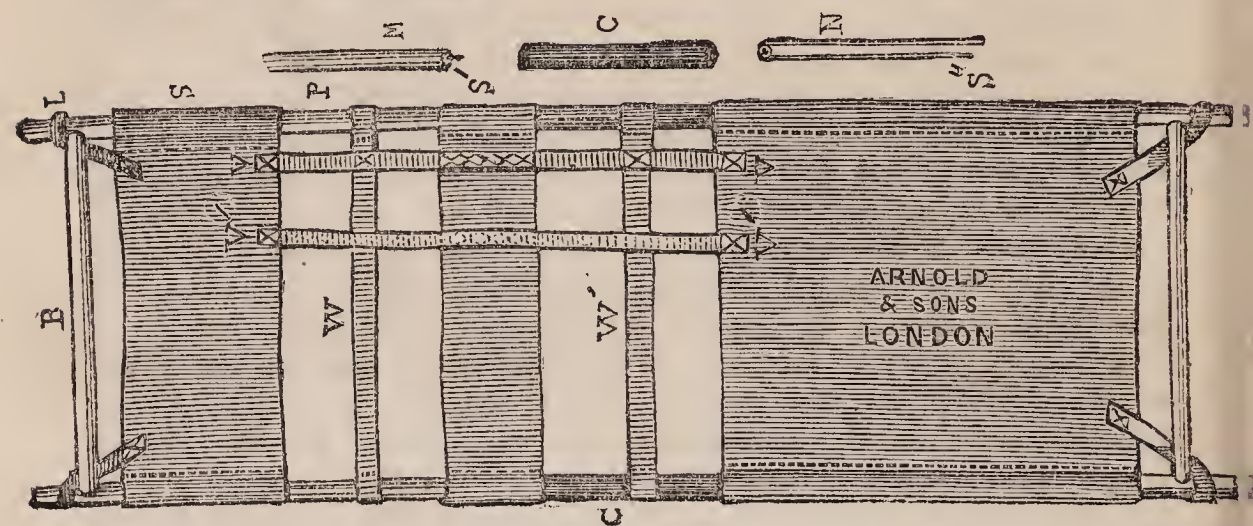
By CHARLES R. B. REETLEY, Esq., Assistant-Demonstrator of Anatomy at St. Bartholomew's Hospital.

In many cases of typhoid and other fevers, of peritonitis, and other inflammations, of bed sore, of various chronic diseases, in many surgical and in obstetric cases, it is very desirable to have a mode of moving a patient from room to room or from bed to bed, and a mode of changing the patient's bed and body-linen without causing the least avoidable disturbance or excitement. For such cases this apparatus has been contrived.

The chief principles on which it is constructed are: 1. It can be placed under the patient piecemeal, and afterwards easily and without danger of mistake fitted together again. 2. It is only a skeleton of webbing in the part which sustains the back and buttocks, and thus allows bedsores and blisters to be easily dressed. 3. The cutaneous sensibility of the back is so extremely blunt that the network of webbing, coarse as it is, feels like a whole and undivided piece.

Its chief advantages are: 1. The most sensitive or feeble patient can be freely moved with no sense of discomfort and no dangerous disturbance. 2. Bedsores and blisters on the back can be easily dressed through its means, and even the back percussed or auscultated through it. 3. Its details can be easily modified by any needle-woman to suit the necessities of par-

particular cases without altering the apparatus in principle. 4. It is light and portable, it packs into a parcel three feet long and five inches in diameter.



In using it the pieces *s*, *s'*, *s''*, *w*, and *w'*, are passed under the patient gently, one by one; and the poles then slipped through the hems and loops. Two chairs, each having one of the cross pieces, *B*, upon it, can sustain the patient while the bed is made, &c.

The stretcher has been used with satisfaction at the Queen's Hospital, Birmingham, and at St. Bartholomew's Hospital, London. It is made by Arnold & Sons, West Smithfield, London, E.C.—*Lancet*, Jan. 8, 1876, p. 49.

INDEX TO VOL. LXXIII.

	PAGE.
Abortion, Prof. Simpson on the complete evacuation of the uterus after ...	275
Acute orchitis, Mr. Nunn on puncture of the testis in ...	206
————— Mr. Henry Smith on puncture of the testis in ...	205
Acute rheumatism, Dr. Lafitte on subcutaneous injection of water in ...	153
————— Dr. MacLagan on salicin in ...	34
————— Ed. of Med. Times and Gazette on salicylic acid in ...	33
Albumen, Dr. Brunton on testing the urine for ...	113
————— Dr. Johnson on the examination of urine for ...	xxviii
————— Dr. Rayne on the examination of urine for ...	xxviii
————— Mr. Dowse on the examination of urine for ...	xxviii
Albuminuria, Dr. Brunton on the pathology and treatment of ...	106
————— Dr. Jamieson on gallic acid in the treatment of ...	114
Alcohol, Dr. Brunton on the physiological action of ...	335
————— in the treatment of disease, Dr. Richardson on ...	330
————— in irregular and intermittent pulse ...	94
<i>Althaus</i> , Dr. J., on the electrolytic dispersion of tumours ...	158
Anæmia, Dr. Brunton on albuminuria as a cause of ...	111
Anæsthetics, Dr. Macleod on the administration of chloroform ...	349
————— Dr. Oxley on the advantages of Dr. Allis's ether inhaler ...	343
————— Dr. Sawyer on ether <i>versus</i> chloroform ...	344
————— Mr. Clover on the administration of ...	347
————— Mr. Tait on Nelaton's inversion method in a chloroform accident ...	348
————— Mr. Tait's new method of administering ether vapour ...	341
————— in operations on the bladder, Sir H. Thompson on ...	198
————— in the operation for extraction of cataract, Dr. Taylor on ...	228
Anatomical preparations, Dr. Craig on hydrate of chloral for preserving ...	363
Aneurism, Mr. Pemberton on the antiseptic ligature of arteries for ...	165
————— aortic, Mr. Annandale's case treated by distal ligature ...	163
Ankle, Mr. West on excision of the ...	119
<i>Annandale</i> , Mr. T., his case of aortic aneurism treated by distal ligature ...	163
Antidote, Dr. du Vivier's officinal multiple ...	327
Antipyretic, Dr. Ewald on salicylic acid as an ...	31
Antiseptic catgut ligatures, Dr. Nankiwel on ...	151
————— catgut ligatures, Editor of Medical Times and Gazette on ...	149
————— catgut ligatures, Prof. Lister on the preparation of ...	123
————— ligature of arteries, Mr. Pemberton on the ...	165
————— ligature of arteries for aneurism, Mr. Annandale on ...	165
————— surgery, Prof. Lister's demonstrations of ...	123
————— treatment, Mr. Smith on Prof. Lister's method of ...	126
————— treatment of open knee-joint, Dr. Cameron on the ...	129
Aquapuncture for the relief of pain, Drs. Lafitte and Lelut on ...	153
————— for the relief of pain, Dr. Griffith on ...	xviii
Arsenical intoxication, chronic, Dr. de Mussy on phosphide of zinc in ...	329
Arteries, Dr. M'Donnell on torsion of ...	166
Asthma, Dr. Oliver on the combined use of morphia and atropia hypodermically in ...	102
————— spasmodic, Dr. Burns on subcutaneous injection of morphia in ...	xxiv
<i>Balfour</i> , Dr. G. W., on intermittent pulse, and on palpitation, cardiac and aortic ...	91
<i>Barlow</i> , Dr. W. H., on the pseudo-hypertrophic paralysis of Duchenne ...	66
Bed-ridden patients, Mr. Reetley's new form of stretcher for removal of ...	367
<i>Bell</i> , Dr. R., on the causation and treatment of diphtheria ...	47
<i>Binz</i> , Dr. C., on the cold water treatment of fever ...	22
Bladder, irritable, Mr. Hewetson's case treated by dilatation of female urethra ...	219
Boracic acid, Dr. Watson on its use in ringworm ...	xlii
Breast, Dr. Buchanan on tumours of the ...	318
<i>Braithwaite</i> , Dr. J., on nitric acid as a caustic in uterine practice ...	316

	PAGE.
British cholera, Dr. Cleland on the use of saccharated lime in ...	16
Bromide of potassium and belladonna in extreme slowness of pulse, with no organic disease ...	98
Bronchocele, Dr. Althaus on the electrolytic treatment of ...	160
<i>Brown-Sequard</i> , Dr. C. E., on paralysis on the side of lesion in the brain ...	74
<i>Brunton</i> , Dr. T. L., on a case of extreme slowness of pulse without organic disease ...	99
———— on the pathology and treatment of albuminuria ...	106
———— on the physiological action of alcohol ...	335
<i>Buchanan</i> , Dr. G., on mammary tumours and their treatment ...	318
Burns, Dr. Craig on hydrate of chloral as an application for ...	364
<i>Calderwood</i> , Dr. G., his case of difficult labour from abnormal rigidity of the os ...	279
<i>Cameron</i> , Dr. H. C., on the antiseptic treatment of open knee-joint ...	129
Cancer, Dr. Buchanan on ...	320
Carbolised catgut ligatures, Dr. Nankiwell on the use of ...	151
———— catgut ligatures, Editor of Med. Times and Gazette on ...	149
Cataract, Dr. Taylor on a case of extraction of ...	226
Catarrh, acute nasal, Dr. Ferrier on the topical use of bismuth in ...	104
Catarrhal inflammation, Dr. Handfield Jones on ...	6
Catheter for bed-ridden patients, Messrs. Salt and Son's improved ...	202
Caustic in uterine practice, Dr. J. Braithwaite on nitric acid as a ...	316
Chloral hydrate, Dr. Craig on the external uses of ...	363
———— Dr. Watson on its use as an antiseptic dressing ...	365
Chloride of lead as a deodorizer and disinfectant, Dr. Goolden on the use of ...	51
Chloroform, Dr. Macleod on the administration of ...	349
———— Mr. Lawson Tait on Nelaton's inversion method in impending death from ...	348
Chronic eczema, Dr. Squire on glycerole of subacetate of lead in ...	245
———— Dr. Wathen on glycerole of subacetate of lead in ...	251
Cleft palate, Sir W. Fergusson on his operation for ...	179
———— Dr. Rawson's safety-needle for approximating the edges in ...	177
<i>Cleland</i> , Prof., on saccharated lime in typhus fever and other complaints ...	16
———— on the use of liquor bismuthi for hemorrhoids and prolapsus ani ...	181
<i>Clover</i> , Mr. J. T., on the administration of anæsthetics ...	347
Concussion of the spine, Mr. Erichsen on ...	355
Cold in the head, Dr. Ferrier on the topical use of bismuth to cure ...	104
———— water treatment of fever, Dr. Binz on ...	22
Conjunctival transplantation from the rabbit, Dr. Wolfe on ...	236
Constipation, obstinate, with symptoms of obstruction, Dr. Burns on subcutaneous injection of morphia in ...	xxvi
<i>Cormack</i> , Sir J. R., on diphtheria ...	44
Cornea, Mr. Solomon on section of, in diseases of inflammatory origin ...	230
<i>Craig</i> , Dr. W., on the external uses of hydrate of chloral ...	363
Cystitis, chronic, Mr. Heath's topical treatment of the female bladder in ...	219
———— following lithotripsy, Sir H. Thompson on the treatment of ...	199
Cysts, ovarian, Dr. Semeleder's treatment by galvano-puncture ...	315
<i>Davy</i> , Mr. R., on puncture of the bladder per rectum in obstinate perineal fistulæ ...	203
Deafness, catarrhal, Dr. Guye's contra-respirator for ...	367
Decubital inflammation, Dr. Handfield Jones on ...	4
Deodoriser, Dr. Goolden on chloride of lead as a ...	51
Diabetes, Dr. Donkin on the skim-milk treatment of ...	115
Diarrhœa, bilious or gouty, Dr. Cleland on saccharated lime in ...	16
Difficult labour, Dr. Calderwood's case of abnormal rigidity of the os uteri ...	279
Digitalis in typhoid fever, Dr. Murrell on the fatal effects of ...	12
Dilatation of the female urethra and neck of bladder, Mr. Heath on ...	218
———— of the female urethra and neck of bladder, Mr. Hewetson's case of ...	219
———— of the female urethra and neck of bladder, Mr. Teale on ...	216
———— of the female urethra, Dr. Edis on the necessity of caution in resorting to forcible ...	222
———— of the female urethra, Prof. Spiegelberg on rapid ...	225

	PAGE.
Diphtheria, Dr. Bell on the causation and treatment of	47
— and scarlatina, Sir J. Rose Cornack on diagnostic distinction	44
of the pellicles of	331
Disease, Dr. Richardson on the total abstinence from alcoholic stimulants in	51
Disinfectant and deodoriser, Dr. Goolden on chloride of lead as a	50
— the universal disinfecting powder	363
Dissecting-room, Dr. Craig on hydrate of chloral for injection of bodies	115
for the	87
<i>Donkin</i> , Dr. A. S., on the skim-milk treatment of diabetes	287
<i>Douglas-Lithgow</i> , Dr. R. A., on nitrite of amyl in nervous cephalalgia	66
<i>Draper</i> , Mr. W., his folding short midwifery forceps	170
Duchenne's paralysis, Dr. Barlow on, and its treatment by electricity	294
<i>Duncan</i> , Dr. J., on the treatment of nævus	290
<i>Duncan</i> , Dr. J. M., on inevitable and other lacerations of orifice of vagina	313
in primiparæ	64
— on the pendulum movement in working the midwifery	
forceps	
<i>Dunlop</i> , Dr. J., on artificial occlusion of the vagina for prolapsus uteri	
Dyspepsia, Dr. Leslie Jones on its treatment by the continuous current	
Ear, Dr. Guye on the danger to, in deaf people of breathing by the mouth	366
Eczema, Dr. Craig on the use of chloral hydrate ointment in	365
— chronic, Dr. Squire on glycerole of subacetate of lead in	245
— Dr. Wathen on glycerole of subacetate of lead in	251
— Dr. Will on the use of salicylic acid in	44
<i>Edis</i> , Dr. A. W., on the necessity of caution in resorting to forcible dilatation	
of the female urethra	222
<i>Editor of British Medical Journal</i> on the inoculability of specific fevers	8
<i>Editor of Medical Times and Gazette</i> on the carbolised catgut ligature	149
Electricity, Dr. Leslie Jones on its employment for the relief of pain	58
Electrolysis, Dr. Althaus on the dispersion of tumours by	158
Enteric fever, Dr. Klein on the contagium of	9
Epididymis, Mr. Jordan on urethral disease from inflammation of	213
Epistaxis, Mr. Cox on injection of liq. ferri perchloridi for	xxii
<i>Erichsen</i> , Mr. J. E., on railway injuries, concussion of the spine, &c.	355
Erythematous inflammation, Dr. Handfield Jones on	7
Ether inhalation, Dr. Sawyer on its advantages over chloroform	344
— inhaler, Dr. Allis's	343
— vapour, Mr. Lawson Tait on a new method of administering	341
Eustachian tube, Prof. Gruber's method of making it pervious and	
inflating the tympanum	240
<i>Ewald</i> , Dr. C. A., on salicylic acid as an antipyretic	31
Excision of the ankle, Mr. West on	119
— of the knee-joint, Mr. Hayes' cases of	144
Extraction of cataract, Dr. Taylor on the operation for	226
Eye, Dr. Craig on hydrate of chloral as a lotion in inflammatory condi-	
tions of	364
— operations, Dr. Taylor's method of illumination for	xliv
Eyeball, Mr. Erichsen on concussion of the	360
— Mr. Walton on examination of, by lateral or oblique illumination	234
Facial neuralgia, Dr. Leslie Jones on electricity in	58
Femur, Mr. Hayes' treatment of shortening and deformity of, consequent	
on fracture	140
— fractured, Dr. Cruise's splint for	143
<i>Fergusson</i> , Sir W., on hare-lip and cleft palate	179
<i>Ferrier</i> , Dr. D., on how to cure a cold in the head	104
Fever, Dr. Binz on the cold water treatment of	22
— Dr. Burdon Sanderson on the excretions in	17
— algid pernicious, Dr. Sullivan on	25
— comatose pernicious, Dr. Sullivan on	27
— malarious, Mr. Hunter on subcutaneous injection of quinine in	55

	PAGE.
Fever, puerperal, Dr. Priestley on	300
— rheumatic, Dr. MacLagan on its treatment by salicin	34
— rheumatic, Editor of Medical Times and Gazette on salicylic acid in	33
— specific, Editor of British Medical Journal on the inoculability of	8
— typhoid, Dr. Murrell on the fatal effects of digitalis in	12
— typhoid, Dr. Klein on the contagium of	9
— typhus, Prof. Cleland on the use of saccharated lime in	16
Fistulæ, perineal, Mr. Davy's instrument for puncture of bladder per rectum in	203
Flatulency and painful dyspepsia treated by electricity	64
Forceps, obstetric, Dr. Johnston on the use of, at the Rotunda Lying-in Hospital during 1875	511
— — — — — Mr. Draper's new folding short forceps	287
Fothergill, Dr. J. M., on sleeplessness and its treatment by hypnotics	72
Fothergill's disease treated by galvanism	60
Fractures, ununited, Dr. Hill's modification of Dieffenbach's operation for	136
— — — — — Mr. Thomas's new operation for	133
Gag for operations about the mouth and tongue, Sir W. Fergusson's	180
Gallic acid in the treatment of albuminuria, Dr. Jamieson on	114
Gleet and incipient stricture, Dr. Otis on the treatment of	207
Godson, Dr. C. on the induction of premature labour	266
Goolden, Dr. R. H. on chloride of lead as a deodoriser and disinfectant	51
Gouty inflammation, Dr. Handfield Jones on	6
Griffith, Dr. G. on the subcutaneous injection of hot water	xix
Gruber, Prof., his method of making pervious the Eustachian tube and of inflating the tympanum	240
Guye, Dr., on the dangers to the ear of breathing by the mouth	366
Hall, Surgeon A. R., on the hypodermic injection of quinine in sunstroke	88
Hare-lip and cleft palate, Sir W. Fergusson's gag for operations in	180
Hayes, Mr. P. J., his cases of excision of the knee-joint	144
— on shortening and deformity of femur consequent on fracture, treated by re-fracture	140
Headaches, Dr. Leslie Jones on electricity in the treatment of	61
— — — — — nervous, Dr. Douglas-Lithgow on nitrite of amyl in	87
Heat apoplexy, subcutaneous injection of quinine in	88
Heath, Mr. C., on dilatation of the female urethra	218
Hemorrhage, from a large vein accidentally wounded, Prof. Lister's plan of stopping	xxii
Hemorrhoids, Prof. Cleland on the use of liquor bismuthi in	182
Hernia, Mr. Millikin's improved truss for	xxvi
Herpes preputialis, Dr. Wathen on lead ointment in	253
Hewetson, Mr. H. B., his case of dilatation of female urethra and neck of bladder for irritability	219
Hill, Dr. M., his new operation for ununited fractures	136
— — — — — Mr. B., on treatment of incipient stricture by Otis's operation	207
Hunter, Surgeon-Major G. Y., on the subcutaneous injection of quinine	55
Hydroceles, Mr. Jordan's method of applying iodine to interior of	259
Hypodermic injection of pure water for relief of pain, Mr. Lucas on the	153
Hypopion, Mr. Solomon on its treatment by ciliary incision	231
Illumination for eye operations, Dr. Taylor's method of	xliv
Incipient stricture, Mr. Hill on its treatment by Otis's operation	207
Incontinence of urine, Mr. Teale on dilatation of neck of female bladder for	216
India-rubber underclothing in psoriasis, Dr. Squire on	258
Induction of premature labour, Dr. Godson's method of	270
Inflammation, Dr. Handfield Jones on some considerations respecting	1
Injections into the urethra, Sir H. Thompson on	184
Inoculability of specific fevers, Editor of British Medical Journal on	8
Insomnia, Dr. Fothergill on hypnotics in	72
Intermittent pulse, Dr. Balfour on	91

	PAGE.
Internal urethrotomy in cases of stricture, Sir H. Thompson on ...	193
Iodine, Mr. Jordan's method of applying to the interior of hydroceles ...	259
<i>Jamieson</i> , Dr. J. T., on gallic acid in the treatment of albuminuria ...	114
<i>Johnston</i> , Dr. G., on the use of the forceps at the Rotunda Lying-in Hospital, Dublin, during 1875 ...	311
<i>Jones</i> , Dr. C. H., on inflammation ...	1
— Dr. L., on electricity as a therapeutic agent for the relief of pain ...	58
<i>Jordan</i> , Mr. F., his method of applying iodine to interior of certain cysts .	259
— on the retention of bougies in continuous dilatation of strictures ...	201
— on urethral disease from inflammation of the epididymis ...	213
<i>Klein</i> , Dr., on the contagium of enteric fever ...	9
Knee-joint, Dr. Cameron on the antiseptic treatment of open ...	129
— Mr. Hayes' cases of excision of the ...	144
<i>Knott</i> , Mr. S. J., his cases of sciatica and neuralgia successfully treated by galvanism ...	85
Labour, difficult, Dr. Calderwood's case of extreme rigidity of os uteri in	279
— premature, Dr. Godson on the various modes of induction of ...	266
Laceration of the orifice of vagina in primiparae, Dr. Duncan on...	294
— of the perineum, Dr. Duncan on ...	296
— of the perineum, Dr. Young on the prevention and treatment of	282
— of the perineum, Mr. Smith on the treatment of ...	284
Lactation, Dr. Lane on a new method of preventing the secretion of milk	314
<i>Lane</i> , Dr. J. W., on the prevention of the secretion of milk ...	314
Ligatures, antiseptic catgut, Prof. Lister's mode of preparing ...	126
<i>Lister</i> , Prof., his demonstrations of antiseptic surgery ...	123
Lithotrity, Sir H. Thompson on some important points in connection with .	196
<i>Lucas</i> , Mr. R. C., on aquapuncture ...	153
Lupus, Dr. Piffard on the treatment of ...	253
<i>MacLagan</i> , Dr. T., on the treatment of acute rheumatism by salicin ...	34
<i>Macleod</i> , Dr. G. H. B., on the administration of chloroform ...	349
Mammary tumours, Dr. Buchanan on the nature and characteristics of ...	318
— Dr. Buchanan on the treatment of ...	321
<i>Maxson</i> , Dr. P. R., his plan of treating shoulder presentations ...	281
<i>McDonnell</i> , Dr. R., on torsion of arteries ...	166
Mercureal tremor, Dr. de Mussy on the use of phosphide of zinc in ..	328
Midwifery forceps, Dr. Duncan against the pendulum movement in working	290
— Dr. Johnston on the use of at the Rotunda Hospital ...	311
— Mr. Draper's short folding ...	290
Milk, Dr. Lanc on the prevention of secretion of ...	314
Miscarriage, Prof. Simpson on the complete evacuation of the uterus after	279
Morphia and atropia, Dr. Oliver on combined use of in spasmodic asthma	102
<i>Murrell</i> , Dr. W., on the fatal effects of digitalis in typhoid fever ...	12
Muscular atrophy, Dr. Onimus on professional ...	362
<i>Mussy</i> , Dr. N. G. de, on phosphide of zinc in mercurial tremor, &c. ...	328
Myalgia, its treatment by the continuous current ...	63
<i>Nævus</i> , Dr. Althaus on the electrolytic treatment of ...	158
— Dr. Duncan on the treatment of ..	170
<i>Nævi</i> , Dr. Duncan on their treatment by electrolysis and by galvanic cautery ...	175
— Dr. Duncan on their treatment by excision, ligature, and ablation	176
— Dr. Duncan on their treatment by injection of coagulating fluids ...	173
— Dr. Duncan on their treatment by setons and subcutaneous ligature	173
<i>Nankivell</i> , Dr. A. W., on the employment of carbolic catgut ligatures ...	151
Neuralgia, Mr. Knott on its treatment by galvanism ...	85

Neuralgia of fifth pair, treated with chloride of ammonium, Dr. Young's case of	84
———— of the face treated by the continuous current... ..	59
———— of right ovary treated with chloride of ammonium, Dr. Young's case of	83
Nitric acid as a caustic in uterine practice, Dr. J. Braithwaite on...	316
Nitrite of amyl in nervous cephalalgia, Dr. Douglas-Lithgow on...	87
Nunn, Mr. T. W., on puncture of the testis in acute orchitis	206
Obstetric forceps, Dr. Duncan against the pendulum movement in working	290
———— Dr. Johnston on cases at the Rotunda Hospital, 1875	311
———— Mr. Draper's short folding	287
Oliver, Dr. G., on combined use of morphia and atropia in spasmodic asthma	102
Onimus, Dr. E., on professional muscular atrophy	362
Ophthalmic-microscope, Mr. Haynes Watson on the	236
Orchitis, acute, Mr. Nunn on puncture of the testis in	206
———— Mr. H. Smith on puncture of the testis in	205
Os uteri, Dr. Calderwood on dilatation of in a case of abnormal rigidity ...	279
Ovarian tumours, Dr. Semeleder's treatment by galvano-puncture	315
Oxley, Dr. M., on Dr. Allis's ether inhaler	343
Pain, Dr. Leslie Jones on the use of electricity for the relief of	58
———— Mr. Lucas on the hypodermic injection of pure water for	153
Palpitation, cardiac, Dr. Balfour on	97
Paralysis, Dr. Brown-Sequard on its appearance on the side of a lesion in the brain... ..	74
———— Duchenne's, Dr. Barlow on the symptoms, pathology, and treatment of	66
Pemberton, Mr. O., on the antiseptic method of ligaturing arteries	165
Pendulum movement in working the midwifery forceps, Dr. Duncan against	290
Perineum, Dr. Duncan on lacerations of the	296
———— Dr. Young on the treatment of lacerations of the	282
———— Mr. Smith on the treatment of rupture of the	284
Perineal fistulæ, obstinate, Mr. Davy on puncture of the bladder per rectum	203
Pernicious fever, Dr. Sullivan on	23
Pessary, Messrs. Salt and Son's new form of	xlvi
Phosphide of zinc in mercurial tremor, &c., Dr. de Mussy on the use of ...	328
Piffard, Dr. H. G., on the treatment of scrofulides (lupus)	253
Poisons, Dr. du Vivier's officinal multiple antidote for	327
Position as a means of rectifying shoulder presentations, Dr. Maxson on .	281
Premature labour, Dr. Godson on the modes of inducing... ..	266
Priestley, Dr. W. O., on puerperal fever... ..	300
Professional muscular atrophy, Dr. Onimus on	362
Prolapsus ani, Prof. Cleland on the use of liquor bismuthi in	181
———— uteri, Dr. Dunlop on artificial occlusion of the vagina for	313
———— vaginæ, Dr. Savage on the surgical treatment of	271
Psoriasis, Dr. Squire on the use of india-rubber underclothing for	258
Pruritus, Dr. Wathen on lead ointment in some forms of... ..	253
Puerperal fever, Dr. Priestley on	300
Pulse, extreme slowness and irregularity of, Dr. Somerville's case of ...	98
Quinine, Surgeon Hall on hypodermic injection of in sunstroke	88
———— Surgeon-Major Hunter on the subcutaneous injection of	55
Railway injuries, Mr. Erichsen on	355
Rawson, Dr. E. A., his safety-needle for closing the edges of a cleft palate .	177
Reetley, Mr. C. R. B., his new form of stretcher for removal of bed-ridden patients	367
Re-fracture of the femur for shortening and deformity, Mr. Hayes's case of	140
Rheumatic fever, Dr. Maclagan on the use of salicin in	34
———— fever, Editor of Medical Times and Gazette on salicylic acid in .	33

	PAGE.
Rheumatic inflammation, Dr. Handfield Jones on	5
Rheumatism, muscular, its treatment by electricity	63
<i>Richardson</i> , Dr. B. W., on alcohol in the treatment of disease	320
Ringworm, Dr. Watson on the use of boracic acid in	xlii
Rupture of the perineum, Dr. Young on the treatment of	282
——— of the perineum, Mr. Smith on the treatment of	284
Saccharated lime, Prof. Cleland on its use in typhus and other complaints	16
Salicin, Dr. MacLagan on its use in acute rheumatism	34
Salicylic acid, Ed. of Med. Times and Gazette on its use in rheumatic fever	33
——— as an antipyretic, Dr. Ewald on	31
——— as an antiseptic, Dr. Ogilvie Will on	40
<i>Sanderson</i> , Dr. J. B., on fever	17
<i>Savage</i> , Dr. T., on the surgical treatment of prolapsus vaginae	271
<i>Sawyer</i> , Dr. J., on the advantages of ether over chloroform	344
Sciatica, Dr. Griffith on the subcutaneous injection of hot water for	xix
——— Dr. Leslie Jones on its treatment by the continuous current	61
——— Mr. Knott on its treatment by galvanism	85
——— Mr. Lucas on a case treated by hypodermic injection of water	155
Scirrhus of the mamma, Dr. Buchanan on a case of	323
Scrofulides, erythematous and corneous, Dr. Piffard on the treatment of	253
——— phlegmonous, Dr. Piffard on the treatment of	258
——— pustular, Dr. Piffard on the treatment of	254
——— tubercular, Dr. Piffard on the treatment of	253
Section of cornea in diseases of inflammatory origin, Mr. Solomon on	230
<i>Semeleder</i> , Dr., his treatment of ovarian tumours by galvano-puncture	315
Shoulder presentations, Dr. Maxson's method of converting into natural presentations	281
<i>Sigmond</i> , Prof., on syphilis	260
<i>Simpson</i> , Prof. A. R., on complete evacuation of the uterus after abortion	275
Skim-milk diet in the treatment of albuminuria, Dr. Donkin on	115
Sleeplessness, Dr. Milner Fothergill on hypnotics in	72
<i>Smith</i> , Mr. H., on puncture of the testis in acute orchitis...	205
<i>Smith</i> , Mr. T., his report on the treatment of ruptured perineum	284
——— on Prof. Lister's antiseptic treatment	126
<i>Solomon</i> , Mr. J. V., on section of the cornea in diseases of the eye of inflammatory origin	230
<i>Somerville</i> , Dr. R., his case of extreme slowness of pulse, without organic heart disease	98
Sore nipples, Dr. Craig on hydrate of chloral as a lotion for	364
Spasmodic asthma, Dr. Oliver on the hypodermic use of morphia and atropia in	102
Specific fevers, Ed. of British Medical Journal on the inoculability of	3
Speculum vaginae, Messrs. Salt & Son's new	265
<i>Spiegelberg</i> , Prof., on rapid dilatation of the urethra in women	225
Spine, Mr. Erichsen on concussion of the	355
Sponge tents, Dr. Godson's instrument for facilitating the introduction of	270
<i>Squire</i> , Dr. B., on the treatment of chronic eczema by glycerole of subacetate of lead	245
——— on the use of india-rubber underclothing in psoriasis	258
Stretcher for the removal of bed-ridden patients, Mr. Reetley's	367
Stricture, Sir H. Thompson on physical examination of the urethra in	186
——— Sir H. Thompson on the best mode of employing instruments in difficult cases	189
——— Sir H. Thompson on the method of performing internal urethrotomy in	193
——— Mr. Jordan on retention of bougies for continuous dilatation of	201
——— incipient, Mr. Hill on its treatment by Otis's operation	207
Subcutaneous injection of water for relief of pain, Mr. Lucas on	153
——— of hot water, Dr. Griffith's syringe for	xix
<i>Sullivan</i> , Dr. J., on pernicious fever: febris algida and febris comatosa	23
Sunstroke, Surgeon Hall on hypodermic injection of quinine in	88
Symblepharon, Dr. Wolfe on conjunctival transplantation from the rabbit	236
Syphilis, Prof. Sigmund's clinical observations on	260

	PAGE.
<i>Tait</i> , Mr. L., on Nelaton's inversion method in a chloroform accident ...	348
----- on a new method of administering ether vapour ...	341
<i>Taylor</i> , Dr. C. B., on a case of cataract extraction ...	226
<i>Teale</i> , Mr. T. P., on dilatation of the urethra and neck of bladder in the female for vesical irritability ...	217
<i>Thomas</i> , Mr. H. O., his treatment of ununited fractures ...	133
<i>Thompson</i> , Sir H., on important points in connection with lithotrity ...	196
----- on diagnosis of diseases of the urinary organs ...	185
----- on employment of instruments in difficult cases of stricture ...	189
----- on internal urethrotomy in cases of stricture ...	193
----- on the physical examination of the urethra in stricture ...	186
----- on the treatment of urethral disease ...	183
Tie douloureux treated by galvanism ...	59
Torsion of arteries, Dr. M'Donnell on ...	166
Truss for hernia, Mr. Millikin's improved ...	xxvi
Tumour, aneurismal, Mr. Annandale on its treatment by distal ligature ...	163
Tumours, Dr. Althaus on the electrolytic dispersion of ...	158
----- mammary, Dr. Buchanan on ...	318
Tympanum, Prof. Gruber on inflation of, and making pervious the Eustachian tube ...	240
Typhoid fever, Dr. Klein on the contagium of ...	9
----- Dr. Murrell on the fatal effects of digitalis in ...	12
----- Prof. Cleland on the use of saccharated lime in ...	16
Ulcers, Dr. Craig on hydrate of chloral as a dressing for ...	364
Ununited fractures, Dr. Hill's modification of Dieffenbach's operation for ...	136
----- Mr. Thomas's treatment of ...	133
Urethra, Sir H. Thompson on making injections into the ...	184
----- Sir H. Thompson on physical examination of in cases of stricture ...	186
----- female, on forcible dilatation of the for irritable bladder ...	216-225
Urethral disease for inflammation of the epididymis, Mr. Jordan on ...	213
Urethrotomy, internal, Sir H. Thompson's mode of performing for stricture ...	195
Urinary organs, Sir H. Thompson on the diagnosis of diseases of the ...	185
Uterine prolapsus, Dr. Dunlop on artificial occlusion of the vagina for ...	313
Uterus, Prof. Simpson on the complete evacuation of after miscarriage ...	275
Vagina, Dr. Duncan on lacerations of the orifice of in primiparæ ...	294
----- Dr. Savage on the treatment of prolapse of the ...	271
Vaginal speculum, Messrs. Salt & Son's novel and compact ...	265
Vesical irritability, Mr. Teale on dilatation of neck of female bladder for ...	216
<i>Vivier</i> , Dr. Du, on an official multiple antidote ...	326
<i>Walton</i> , Mr. H., on examination of the eyeball by lateral or oblique illumination ...	234
<i>Warren</i> , Dr. E., on rectification of shoulder presentations by position ...	281
Warts, Dr. Craig on hydrate of chloral as an application to remove ...	364
<i>Wathen</i> , Dr. J. H., on glycerole of sub-acetate of lead in chronic eczema ...	251
<i>Watson</i> , Dr. P. H., on the use of chloral hydrate as an antiseptic dressing ...	365
<i>West</i> , Mr. J. F., on excision of the ankle ...	119
<i>Will</i> , Dr. C. J. O., on salicylic acid ...	40
<i>Wolfe</i> , Dr. J. R., on conjunctival transplantation from the rabbit ...	236
Wounds, Dr. Watson on chloral hydrate as a dressing for ...	365
----- and abscesses, Mr. Smith on the antiseptic treatment of ...	126
<i>Young</i> , Dr. D., his cases of neuralgia treated with choride of ammonium ...	83
<i>Young</i> , Dr. J., on the treatment of laceration of the perineum ...	282



